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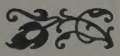

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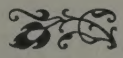

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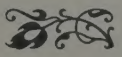

Testability and Meaning—*Continued*

BY

RUDOLF CARNAP

IV. THE CONSTRUCTION OF A LANGUAGE-SYSTEM

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IV. THE CONSTRUCTION OF A LANGUAGE-SYSTEM

17. The Problem of a Criterion of Meaning

IT IS not the aim of the present essay to defend the principle of empiricism against apriorism or anti-empiricist metaphysics. Taking empirism[1] for granted, we wish to discuss, the question what is meaningful. The word 'meaning' will here be taken in its empiricist sense; an expression of language has meaning in this sense if we know how to use it in speaking about empirical facts, either actual or possible ones. Now our problem is what expressions are meaningful in this sense. We may restrict this question to sentences because expressions other than sentences are meaningful if and only if they can occur in a meaningful sentence.

Empiricists generally agree, at least in general terms, in the view that the question whether a given sentence is meaningful is closely connected with the questions of the possibility of verification, confirmation or testing of that sentence. Sometimes the two questions have been regarded as identical. I believe that this identification can be accepted only as a rough first approximation. Our real problem now is to determine the precise relation between the two questions, or generally, to state the criterion of meaning in terms of verification, confirmation or testing.

I need not emphasize that here we are concerned only with the problem of meaning as it occurs in methodology, epistemology or applied logic,[2] and not with the psychological question of meaning. We shall not consider here the questions whether any images and, if so, what images are connected with a given sentence. That these questions belong to psychology and do not touch the methodological question of meaning, has often been emphasized.[3]

[1] The words 'empiricism' and 'empiricist' are here understood in their widest sense, and not in the narrower sense of traditional positivism or sensationalism or any other doctrine restricting empirical knowledge to a certain kind of experience.

[2] Our problem of meaning belongs to the field which *Tarski* [1] calls *Semantic*; this is the theory of the relations between the expressions of a language and things, properties, facts etc. described in the language.

[3] Comp. e.g. Schlick [4] p. 355.

It seems to me that the question about the criterion of meaning has to be construed and formulated in a way different from that in which it is usually done. In the first place we have to notice that this problem concerns the structure of language. (In my opinion this is true for all philosophical questions, but that is beyond our present discussion.) Hence a clear formulation of the question involves reference to a certain language; the usual formulations do not contain such a reference and hence are incomplete and cannot be answered. Such a reference once made, we must above all distinguish between two main kinds of questions about meaningfulness; to the first kind belong the questions referring to a historically given language-system, to the second kind those referring to a language-system which is yet to be constructed. These two kinds of questions have an entirely different character. A question of the first kind is a theoretical one; it asks, what is the actual state of affairs; and the answer is either true or false. The second question is a practical one; it asks, how shall we procede; and the answer is not an assertion but a proposal or decision. We shall consider the two kinds one after the other.

A *question of the first kind* refers to a given language-system L and concerns an expression E of L (i.e. a finite series of symbols of L). The question is, whether E is meaningful or not. This question can be divided into two parts: a) "Is E a sentence of L"?, and b) "If so, does E fulfill the empiricist criterion of meaning"? Question (a) is a formal question of logical syntax (comp. Chapter II); question (b) belongs to the field of methodology (comp. Chapter III). It would be advisable to avoid the terms 'meaningful' and 'meaningless' in this and in similar discussions – because these expressions involve so many rather vague philosophical associations – and to replace them by an expression of the form "a . . . sentence of L"; expressions of this form will then refer to a specified language and will contain at the place '. . .' an adjective which indicates the methodological character of the sentence, e.g. whether or not the sentence (and its negation) is verifiable or completely or incompletely confirmable or completely or incompletely testable and the like, according to what is intended by 'meaningful'.

18. The Construction of a Language-System L

A *question of the second kind* concerns a language-system L which is being proposed for construction. In this case the rules of L are not given, and the problem is how to choose them. We may construct L in whatever way we wish. There is no question of right or wrong, but only a practical question of convenience or inconvenience of a system form, i.e. of its suitability for certain purposes. In this case a theoretical discussion is possible only concerning the consequences which such and such a choice of rules would have; and obviously this discussion belongs to the first kind. The special question whether or not a given choice of rules will produce an empiricist language, will then be contained in this set of questions.

In order to make the problem more specific and thereby more simple, let us suppose that we wish to construct L as a physical language, though not as a language for all science. The problems connected with specifically biological or psychological terms, though interesting in themselves, would complicate our present discussion unnecessarily. But the main points of the philosophical discussions of meaning and testability already occur in this specialized case.

In order to formulate the rules of an intended language L, it is necessary to use a language L′ which is already available. L′ must be given at least practically and need not be stated explicitly as a language-system, i.e. by formulated rules. We may take as L′ the English language. In constructing L, L′ serves for two different purposes. First, L′ is the syntax-language[4] in which the rules of the object-language L are to be formulated. Secondly, L′ may be used as a basis for comparison for L, i.e. as a first object-language with which we compare the second object-language L, as to richness of expressions, structure and the like. Thus we may consider the question, to which sentences of the English language (L′) do we wish to construct corresponding sentences in L, and to which not. For example, in constructing the language of Principia Mathematica, Whitehead and Russell wished to have

[4] Comp. Carnap [4] §1; [5], p. 39.

available translations for the English sentences of the form "There is something which has the property φ"; they therefore constructed their language-system so as to contain the sentence-form "$(\exists x) \cdot \varphi x$". A difficulty occurs because the English language is not a language-system in the strict sense (i.e. a system of fixed rules) so that the concept of translation cannot be used here in its exact syntactical sense. Nevertheless this concept is sufficiently clear for our present practical purpose. The comparison of L with L′ belongs to the rather vague, preliminary considerations which lead to decisions about the system L. Subsequently the result of these decisions can be exactly formulated as rules of the system L.

It is obvious that we are not compelled to construct L so as to contain sentences corresponding to all sentences of L′. If e.g. we wish to construct a language of economics, then its sentences correspond only to a small part of the sentences of the English language L′. But even if L were to be a language adequate for all science there would be many – and I among them – who would not wish to have in L a sentence corresponding to every sentence which usually is considered as a correct English sentence and is used by learned people. We should not wish e.g. to have corresponding sentences to many or perhaps most of the sentences occurring in the books of metaphysicians. Or, to give a non-metaphysical example, the members of our Circle did not wish in former times to include into our scientific language a sentence corresponding to the English sentence

S_1: "This stone is now thinking about Vienna."

But at present I should prefer to construct the scientific language in such a way that it contains a sentence S_2 corresponding to S_1. (Of course I should then take S_2 as false, and hence $\sim S_2$ as true.) I do not say that our former view was wrong. Our mistake was simply that we did not recognize the question as one of decision concerning the form of the language; we therefore expressed our view in the form of an assertion – as is customary among philosophers – rather than in the form of a proposal. We used to say: "S_1 is not false but meaningless"; but the careless use of the word

'meaningless' has its dangers and is the second point in which we would like at present to modify the previous formulation.

We return to the question how we are to proceed in constructing a physical language L, using as L' the English physical language.

The following list shows the items which have to be decided in constructing a language L.

I. Formative rules (= definition of 'sentence in L').
- A. Atomic sentences.
  - 1. The form of atomic sentences.
  - 2. The atomic predicates.
    - a. Primitive predicates.
    - b. Indirectly introduced atomic predicates.
- B. Formative operations of the first kind: Connections; Molecular sentences.
- C. Formative operations of the second kind: Operators.
  - 1. Generalized sentences. (This is the *critical point.*)
  - 2. Generalized predicates.

II. Transformative rules (= definition of 'consequence in L').
- A. L-rules. (The rules of logical deduction.)
- B. P-rules. (The physical laws stated as valid.)

In the following sections we shall consider in succession items of the kind I, i.e. the formative rules. We will choose these rules for the language L from the point of view of empiricism; and we shall try, in constructing this empiricist language L, to become clear about what is required for a sentence to have meaning.

19. Atomic Sentences: Primitive Predicates

The suitable method for stating formative rules does not consist in describing every single form of sentence which we wish to admit in L. That is impossible because the number of these forms is infinite. The best method consists in fixing

1. The forms of some sentences of a simple structure; we may call them (elementary or) *atomic sentences* (I A);

2. Certain *operations* for the formation of compound sentences (I B, C).

I A 1. Atomic sentences. As already mentioned, we will consider only predicates of that type which is most important for

physical language, namely those predicates whose arguments are individual constants i.e. designations of space-time-points. (It may be remarked that it would be possible and even convenient to admit also full sentences of physical functors as atomic sentences of L, e.g. 'te(a) = r', corresponding to the sentence of L': "The temperature at the space-time-point *a* is r". For the sake of simplicity we will restrict the following considerations to predicate-sentences. The results can easily be applied to functor-sentences also.) An atomic sentence is a full sentence of an atomic predicate (Definition 15a, §9). An atomic predicate is ether primitive or introduced by an atomic chain (Definition 14b, §9). Therefore we have to answer the following questions in order to determine the form of the atomic sentences of L:

I A 2. a) Which predicates shall we admit as primitive predicates of L?

b) Which forms of atomic introductive chains shall we admit?

I A 2a: *Primitive predicates.* Our decision concerning question (a) is obviously very important for the construction of L. It might be thought that the richness of language L depends chiefly upon how rich is the selection we make of primitive predicates. If this were the case the philosophical discussion of what sentences were to be included in L – which is usually formulated as: what sentences are meaningful? – would reduce to this question of the selection of primitive predicates. But in fact this is not the case. As we shall see, the main controversy among philosophers concerns the formation of sentences by operators (I C 1). About the selection of primitive predicates agreement can easily be attained, even among representatives of the most divergent views regarding what is meaningful and what is meaningless. This is easily understood if we remember our previous considerations about sufficient bases. If a suitable predicate is selected as the primitive predicate of L, all other physical predicates can be introduced by reduction chains.

To illustrate how the selection of primitive predicates could be carried out, let us suppose that the person N_1 who is constructing the language L trusts his sense of sight more than his other senses. That may lead him to take the colour-predicates (attributed to

things or space-time-points, not to acts of perception, compare the example given on p. 466, vol. 3) as primitive predicates of L. Since all other physical predicates are reducible to them, N_1 will not take any other primitive predicates. It is just at this point in selecting primitive predicates, that N_1 has to fact the question of observability. If N_1 possesses a normal colour sense each of the selected predicates, e.g. 'red', is observable by him in the sense explained before (§ 11). Further, if N_1 wishes to share the language L with other people – as is the case in practice – N_1 must inquire whether the predicates selected by him are also observable by them; he must investigate whether they are able to use these predicates in sufficient agreement with him, – whether it be subsequent to training by him or not. We may suppose that N_1 will come to a positive result on the basis of his experience with English-speaking people. Exact agreement, it is true, is not obtainable; but that is not demanded. Suppose however that N_1 meets a completely colour-blind man N_2. N_1 will find that he cannot get N_2 to use the colour predicates in sufficient agreement with him, in other words, that these predicates are not observable by N_2. If nevertheless N_1 wishes to have N_2 in his language-community, N_1 must change his selection of primitive predicates. Perhaps he will take the brightness-predicates which are also observable by him. But there might be a completely blind man N_3, for whom not one of the primitive predicates selected by N_1 is observable. Is N_3 now unable to take part in the total physical language of N_1? No, he is not. N_1 and N_3 might both take e.g. the predicate 'solid' as primitive predicate for their common language L. This predicate is observable both for N_3 and N_1, and it is a sufficient confirmation basis for the physical language L, as we have seen above. Or, if N_1 prefers to keep visual predicates as primitive predicates for L, he may suggest to N_3 that he take 'solid' as primitive predicate of N_3's language L_3 and then introduce the other predicates by reduction in such a way that they agree with the predicates of N_1's language L. Then L and L_3 will be completely congruent even as to the stock of predicates, though the selections of primitive predicates are different. How

far N_1 will go in accepting people with restricted sensual faculties into his language-community, is a matter of practical decision. For our further considerations we shall suppose that only observable predicates are selected as primitive predicates of L. Obviously this restriction is not a necessary one. But, as empiricists, we want every predicate of our scientific language to be confirmable, and we must therefore select observable predicates as primitive ones. For the following considerations we suppose that the primitive predicates of L are observable without fixing a particular selection.

Decision 1. Every primitive descriptive predicate of L is observable.

20. The Choice of a Psychological or a Physical Basis

In selecting the primitive predicates for the physical language L we must pay attention to the question whether they are observable, i.e. whether they can be directly tested by perceptions. Nevertheless we need not demand the existence of sentences in L – either atomic or other kinds – corresponding to perception-sentences of L′ (e.g. "I am now seeing a round, red patch"). L may be a physical language constructed according to the demands of empiricism, and may nevertheless contain no perception-sentences at all.

If we choose a basis for the whole scientific language and if we decide as empiricists, to choose observable predicates, two (or three) different possibilities still remain open for specifying more completely the basis, apart from the question of taking a narrower or wider selection. For, if we take the concept 'observable' in the wide sense explained before (§ 11) we find two quite different kinds of observable predicates, namely physical and psychological ones.

1. Observable *physical predicates of the thing-language*, attributed to perceived things of any kind or to space-time-points. All examples of primitive predicates of L mentioned before belong to this kind. Examples of full sentences of such predicates: "This thing is brown," "This spot is quadrangular," "This space-time-point is warm," "At this space-time-point is a solid substance."

2. Observable *psychological predicates*. Examples: "having a feeling of anger," "having an imagination of a red triangle," "being in the state of thinking about Vienna," "remembering the city hall of Vienna." The perception predicates also belong to this kind, e.g. "having a perception (sensation) of red," ". . . of sour"; these perception predicates have to be distinguished from the corresponding thing-predicates belonging to the first kind (see vol. 3, p. 466). These predicates are observable in our sense in so far as a person N who is in such a state can, under normal conditions, be aware of this state and can therefore directly confirm a sentence attributing such a predicate to himself. Such an attribution is based upon that kind of observation which psychologists call introspection or self-observation, and which philosophers sometimes have called perception by the inner sense. These designations are connected with and derived from certain doctrines to which I do not subscribe and which will not be assumed in the following; but the fact referred to by these designations seems to me to be beyond discussion. Concerning these observable psychological predicates we have to distinguish two interpretations or modes of use, according to which they are used either in a phenomenological or in a physicalistic language.

2a. Observable psychological predicates *in a phenomenological language*. Such a predicate is attributed to a so-called state of consciousness with a temporal reference (but without spatial determination, in contradistinction to 2b). Examples of full sentences of such predicates (the formulation varies according to the philosophy of the author): "My consciousness is now in a state of anger" (or: "I am now . . .," or simply: "Now anger"); and analogously with "such and such an imagination," ". . . remembrance," ". . . thinking," ". . . perception," etc. These predicates are here interpreted as belonging to a phenomenological language, i.e. a language about conscious phenomena as non-spatial events. However, such a language is a purely subjective one, suitable for soliloquy only, while the intersubjective thing-language is suitable for use among different subjects. For the construction of a subjective language predicates of this kind may be taken as primitive predicates. Several such subjective

languages constructed by several subjects may then be combined for the construction of an intersubjective language. But the predicates of this kind cannot be taken directly as observable primitive predicates of an intersubjective language.

2b. Observable psychological predicates *in a physicalistic language*. Such a predicate is attributed to a person as a thing with spatio-temporal determination. (I believe that this is the use of psychological predicates in our language of everyday life, and that they are used or interpreted in the phenomenological way only by philosophers.) Examples of full sentences: "Charles was angry yesterday at noon," "I (i.e. this person, known as John Brown) have now a perception of red," etc. Here the psychological predicates belong to an intersubjective language. And they are intersubjectively confirmable. N_2 may succeed in confirming such a sentence as "N_1 is now thinking of Vienna" (S), as is constantly done in everyday life as well as in psychological investigations in the laboratory. However, the sentence S is confirmable by N_2 only incompletely, although it is completely confirmable by N_1. [It seems to me that there is general agreement about the fact that N_1 can confirm more directly than N_2 a sentence concerning N_1's feelings, thoughts, etc. There is disagreement only concerning the question whether this difference is a fundamental one or only a difference in degree. The majority of philosophers, including some members of our Circle in former times, hold that the difference is fundamental inasmuch as there is a certain field of events, called the consciousness of a person, which is absolutely inaccessible to any other person. But we now believe, on the basis of physicalism, that the difference, although very great and very important for practical life, is only a matter of degree and that there are predicates for which the directness of confirmation by other persons has intermediate degrees (e.g. 'sour' and 'quadrangular' or 'cold' when attributed to a piece of sugar in my mouth). But this difference in opinion need not be discussed for our present purposes.] We may formulate the fact mentioned by saying that the psychological predicates in a physicalistic language are intersubjectively confirmable but only *subjectively observable*. [As to testing, the difference is still

greater. The sentence S is certainly not completely testable by N_2; and it seems doubtful whether it is at all testable by N_2, although it is certainly confirmable by N_2.] This feature of the predicates of kind 2b is a serious disadvantage and constitutes a reason against their choice as primitive predicates of an inter-subjective language. Nevertheless we would have to take them as primitive predicates in a language of the whole of science if they were not reducible to predicates of the kind 1, because in such a language we require them in any case. But, if physicalism is correct they are in fact reducible and hence dispensable as primitive predicates of the whole language of science. And certainly for the physical language L under construction we need not take them as primitive.

According to these considerations, it seems to be preferable to choose the primitive predicates from the predicates of kind 1, i.e. of the observable thing-predicates. These are the only inter-subjectively observable predicates. In this case, therefore, the same choice can be accepted by the different members of the language community. We formulate our decision concerning L, as a supplement to Decision 1:

Decision 2. Every primitive predicate of L is a thing predicate.

The choice of primitive predicates is meant here as the choice of a basis for possible confirmation. Thus, in order to find out whether the choice of primitive predicates of the kind 1 or 2a or 2b corresponds to the view of a certain philosopher, we have to examine what he takes as the basis for empirical knowledge, for confirmation or testing. *Mach*, by taking the sensation elements ('Empfindungselemente') as basis, can be interpreted as a representative of the standpoint 2a; and similarly other positivists, sensationalists and idealists. The views held in the first period of the Vienna Circle were very much influenced by positivists and above all by Mach, and hence also show an inclination to the view 2a. I myself took elementary experiences ('Elementarerlebnisse') as basis, (in [1]). Later on, when our Circle made the step to physicalism, we abandoned the phenomenological language recognizing its subjective limitation.[5] *Neurath*[6] requires for the basic

[5] Comp. Carnap [2], §6.
[6] Neurath [5] and [6] p. 361.

sentences ('Protokollsätze'), i.e. those to which all confirmation and testing finally goes back, the occurrence of certain psychological terms of the kind 2b—or: of biological terms, as we may say with Neurath in order to stress the physicalistic interpretation—namely designations of actions of perception (as physicalistic terms). He does not admit in these basic sentences such a simple expression as e.g. "a black round table" which is observable in our sense but requires instead "a black round table perceived (or: seen) by Otto." This view can perhaps be interpreted as the choice of predicates of the kind 2b as primitive ones. We have seen above the disadvantages of such a choice of the basis. *Popper*[7] rejects for his basic sentences reference to mental events, whether it be in the introspective, phenomenological form, or in physicalistic form. He characterizes his basic sentences with respect to their form as singular existential sentences and with respect to their content as describing observable events; he demands that a basic sentence must be intersubjectively testable by observation. Thus his view is in accordance with our choice of predicates of the kind 1 as primitive ones. He was, it seems to me, the first to hold this view. (The only inconvenient point in his choice of basic sentences seems to me to be the fact that the negations of his basic sentences are not basic sentences in his sense.)

I wish to emphasize the fact that I am in agreement with Neurath not only in the general outline of empiricism and physicalism but also in regard to the question what is to be required for empirical confirmation. Thus I do not deny—as neither Popper nor any other empiricist does, I believe—that a certain connection between the basic sentences and our perceptions is required. But, it seems to me, it is sufficient that the biological designations of perceptive activity occur in the sormulation of the methodological requirement concerning the basic fentences—as e.g. in our formulation "The primitive descriptive predicates have to be observable," where the term "observable" is a biological term referring to perceptions—and that they need not occur in the basic sentences themselves. Also a language restricted to physics as e.g. our language L without containing any biological or perception terms may be an empiricist language provided its primitive descriptive predicates are observable; it may even fulfill the requirement of empiricism in its strictest form inasmuch as all predicates are completely testable. And this language is in its nature quite different from such a language as e.g. that of theoretical physics. The latter language—although as a part

[7] Popper [1] p. 58 f.

of the whole language of science, it is an empiricist language because containing only confirmable terms—does not contain observable predicates of the thing-language and hence does not include a confirmation basis. On the other hand, a physical language like L contains within itself its basis for confirmation and testing.

21. Introduced Atomic Predicates

Beside the question just discussed concerning the choice of a psychological or a physical basis no problems of a fundamental, philosophical nature arise in selecting primitive predicates. In practice, an agreement about the selection can easily be obtained, because every predicate whose observability could be doubted—as e.g. electric field or the like – can easily be dispensed with. As mentioned before, the whole situation described here is not logically necessary, but a contingent character of the system of predicates in their relation to reducibility and consequently to the laws of science. This character of the system of science explains the historical fact that nearly all controversies among contemporary philosophers – at least among those who reject trans-empirical speculative metaphysics – about the limitation of language do not concern the selection of primitive predicates but the selection of formative operations to be admitted. These operations will be considered later on.

As we have seen, the question of observability has to be decided only for the predicates to be chosen as primitive predicates. Our description of the process of their selection has shown that it is an empirical question, not a logical one. All other questions of confirmability of a given predicate concern indirect confirmation, which depends upon the logical, i.e. syntactical relations between the predicate in question and observable predicates. Thus these further questions of confirmability concern the structure of the language, namely the form of definitions and reduction sentences. However, the question of testability of a given predicate involves, in addition, another empirical question, namely whether certain confirmable predicates are realizable.

I A2b. Indirectly introduced atomic predicates. In addition to the primitive predicates of the physical language L other predi-

cates have to be introduced by introductive chains. We have to decide – first for atomic predicates, and later on also for predicates of other kinds – whether to admit in introductive chains definitions only, or also reduction sentences of the general form. In our previous considerations we have seen that the introduction by reduction is practically indispensable. Therefore we decide to admit it. There are two possibilities: we may or may not restrict the introductive chains in L to test chains. We will leave this point undecided and formulate the two possible forms of our decision:

Decision 3. Introductive chains containing reduction pairs are admitted in L,
either a) *only* in the form of *test chains*,
or b) without restriction to test chains.

Theorem 15. If the primitive predicates of a language are observable – as e.g. in our language L according to Decision 1 —all atomic predicates are completely confirmable; moreover, they are completely testable if only test chains are admitted – as e.g. in L in the case of Decision 3a. – This follows from Theorem 8 (§ 12).

22. Molecular Sentences

After considering the question of the atomic sentences of L (I A in the list of p. 6), we have to consider the second part of the formative rules, namely the rules determining what operations for the formation of compound sentences are to be admitted. We have to distinguish two main kinds of such operations:

1) the formation of molecular sentences with the aid of connections (I B);
2) the formation of generalized sentences with the aid of operators (I C).

IB: Connections. There are two kinds of sentential connections. The so-called extensional connections or truth-functions are characterized by the fact that the truth-value of any compound sentence constructed with their help depends only upon the truth-values of the component sentences. The connections of the usual sentential calculus mentioned before are extensional

(see § 5): negation, disjunction, conjunction, implication, equivalence. The non-extensional connections are called intensional[8]; to them belong e.g. Lewis' strict implication[9] and the so-called modal functions.[10] In the case of an intensional connection the truth-value of a compound sentence depends upon the truth-values as well as the forms of the component sentences. (Here it is presupposed that sufficient L-rules are stated for the connective symbol in question; if that is not the case the symbol is, strictly speaking, not a logical, but a descriptive one[11] and hence would have to be introduced on the basis of the primitive descriptive predicates.)

That the extensional connections are admissible and even necessary (at least a sufficient selection of one or two of them by which the others can be defined if desired) is not in doubt. But whether or not they are sufficient, i.e. whether or not intensional connections are also desirable or perhaps necessary for the expressiveness of the language, is still discussed by logicians. I believe that we can dispense with them without making the language poorer.[12] However, the question is not important for our present problem concerning meaningfulness, because those who prefer not to introduce the connections of this kind, do not deny that they are meaningful.

For the sake of simplicity we will not use intensional connections in language L.

Decision 4. The sentential connections in L are extensional. This decision seems to be justified by the fact that so far no concept needed for a language of science is known which could not be expressed in a language having extensional connections only; e.g. the concept of probability can also be expressed extensionally. Of course this decision is here made only for the language L as an

[8] For the lack of better terms I keep Russell's terms 'extensional' and 'intensional'; it is to be noticed that here they have only the above given meaning, not the meaning they have in traditional philosophy.

[9] C. I. Lewis and C. H. Langford [1].

[10] Comp. Carnap [4] §69.

[11] Comp. Carnap [4] §50 and 62.

[12] Comp. Carnap [4] §70.

object of our present considerations and does not at all intend to dispose of the whole problem. – The restriction to extensional predicates was presupposed in our former definitions of 'molecular form', 'molecular predicate', 'molecular sentence'; hence these definitions can now be applied to L.

Theorem 16. If the primitive predicates of a language are observable – as they are e.g. in L according to Decision 1 – the following is true. a. All molecular predicates are completely confirmable and all molecular sentences are bilaterally completely confirmable. b. If only test chains are admitted – as e.g. in L in the case of Decision 3a – all molecular predicates are completely testable and all molecular sentences are bilaterally completely testable. This follows from Theorems 8 and 9 (§ 12).

A universal or existential sentence which is restricted to a finite field (as e.g. the sentences constructed with restricted operators in the languages I and II dealt with in Carnap [4]) can be transformed into a conjunction or a disjunction respectively and therefore has the same character as a molecular sentence. It is also completely confirmable, if the predicates occurring are completely confirmable. If such sentences occurred in L it would be convenient to include them among the molecular sentences. But we will suppose that L does not contain sentences of this kind.

23. Molecular Languages

The fact that the molecular sentences are completely confirmable and, in the case of Decision 3a, also completely testable, is an important advantage of these sentences over the essentially generalized sentences. Let us call a language limited to molecular sentences exclusively, a *molecular language.* Such a language fulfills the requirements of confirmability and testability in its most radical form. Hence we understand the fact that certain epistemologists, especially positivists, propose or demand a molecular language as the language of science. We shall regard as examples the views of Russell, Wittgenstein, Schlick and Ramsey.

In a molecular language unrestricted universality cannot be expressed. Therefore, if such a language is chosen, we have to face the problem of how to deal with the physical laws. There

seem to be in the main two possible ways. A law may be expressed in the form of a molecular sentence, namely a restricted universal sentence or a conjunction, concerning those instances of the law which have been observed so far. On the other hand a law may be taken, not as a sentence, but as a rule of inference according to which one molecular sentence (e.g. a prediction about a future event) can be inferred from other ones (e.g. sentences about observed events). Each of these ways has actually been followed, as we shall see.

Russell asserts the following thesis in discussing the "question of the verifiability of physics"[13]: "Empirical knowledge is confined to what we actually observe."[14] This view is perhaps influenced by *Mach's* positivism.[15] If we wish to interpret this thesis we have to make it clearer by translating it from the material idiom into a formal (or a semi-formal) one (comp. § 4): "The assertions of empirical science are confined to those sentences which are deducible from stated observation-sentences" (i.e. from sentences about actual observations). As this thesis is true for a molecular language of a certain kind, but not for a language containing physical laws in the form of unrestricted universal sentences, we may interpret Russell's view as presupposing a molecular language.

Wittgenstein, perhaps influenced by Mach and Russell, requires that every sentence must be completely verifiable.[16] Thus we might expect him to acknowledge as legitimate only a molecular language. And indeed he asserts that "propositions are truth-functions of elementary propositions,"[17] "all propositions are results of truth-operations on the elementary propositions";[18] here truth-functions are conceived as not including general operators.[19] In consequence of this, Wittgenstein does not acknowledge physical laws as sentences in the proper sense, but takes them as rules for forming (or rather, stating) sentences, thus choosing the

[13] Russell [2] p. 110.
[14] l.c., p. 112.
[15] Comp. l.c., p. 123.
[16] Comp. *Waismann* [1], p. 229.
[17] Wittgenstein [1], prop. 5, p. 103.
[18] l.c., prop. 5.3, p. 119.
[19] l.c., prop. 5.521, p. 135.

second of the two ways mentioned above. This view of Wittgenstein is reported by *Schlick* who is himself in agreement with it.[20]

Ramsey propounds a quite similar view, perhaps influenced by Wittgenstein. A universal sentence like "All men are mortal" – he calls it a variable hypothetical – is not a conjunction, because "it cannot be written out as one";[21] "if then it is not a conjunction, it is not a proposition at all";[22] "variable hypotheticals are not judgments, but rules for judging 'If I meet a ϕ, I shall regard it as a ψ' ";[23] a variable hypothetical "is not strictly a proposition at all, but a formula from which we derive propositions."[24]

Previously, influenced also by Mach and Russell, I too accepted a molecular language.[25] According to the positivistic principle of testability in its most radical form, I restricted the atomic sentences to sentences about actual experiences. The laws of physics as well as all predictions were interpreted as records of present and (remembered) past experiences, namely those experiences from which the law or the prediction is usually said to be inferred by induction. Thus I followed the first of the two ways mentioned above; the physical laws also were interpreted as molecular sentences. At present I no longer hold this view. But I do not think – as Lewis and Schlick do – that it was false. I think it is

[20] *Schlick* [1] p. 150: "A definitive verification" of a natural law "is, strictly speaking, impossible"; it follows from this that a law, "logically considered, does not have the character of an assertion, for a genuine assertion must admit of being definitively verified." It follows from the fact "that one can never actually speak of an absolute verification of a natural law" that "a natural law essentially does not possess the logical character of an 'assertion,' but rather presents an 'instruction for the formation of assertions' (I am indebted to Ludwig Wittgenstein for these ideas and terms)" (l. c. p. 151). "Instructions of this kind occur grammatically in the guise of ordinary sentences." By this explanation, "the problem of induction becomes pointless," i.e. "the question of the logical justification of universal sentences about reality." "We recognize with Hume that there is no logical justification for them; there can be none because they are not genuine sentences. Natural laws are not 'general implications' (to use the language of the logician); because they cannot be verified for *all* cases; rather, they are prescriptions, rules of procedure for the investigator to discover true sentences" (l. c. p. 156).

[21] Ramsey [1], p. 237.

[22] l.c., p. 238.

[23] l.c., p. 241.

[24] l.c., 251.

[25] Carnap [1].

true concerning a molecular language (of a special kind). But I was wrong in thinking that the language I dealt with was *the* language, i.e. the only legitimate language, – as Wittgenstein, Schlick and Lewis likewise seem to think concerning the language-forms accepted by them. Consequently I made the mistake of formulating my epistemological view in the form of an assertion – as most philosophers do – instead of in the form of a suggestion concerning the form of language. At present I think that the whole question is a matter of choice, of convention; and further, that a molecular language can be chosen as the language of science, but that a non-molecular, generalized one is much more suitable and, in addition, closer to the actual practice of science. This will soon be explained.

It may be mentioned that in the discussion about the logical foundations of mathematics, some finitists or intuitionists, e.g. *Weyl*, *Brouwer* and *Kaufmann*, sometimes express opinions which are related to those just quoted and which may be understood as arguing in favor of a molecular language. Thus for instance *Kaufmann*[26] rejects unrestricted universal sentences (except the *a priori* ones), because they are not verifiable. In *Weyl's*[27] opinion a pure existential judgment (as he calls it) is not a proper judgment, but a 'judgment-abstract', similar to a description of a hidden treasure without indication of its place; and a universal judgment is not a proper judgment, but a rule for judgments ('Urteilsanweisung'). We will not analyse here the views of these authors in detail, because they are chiefly concerned with mathematics rather than empirical science.

24. The Critical Problem: Universal and Existential Sentences

So far we have considered the first kind of operations by which compound sentences may be constructed out of atomic sentences, namely the construction of molecular sentences by the help of connections. Now we have to deal with the second kind of operations (I c in the list of p. 6), namely the construction of generalized sentences with the aid of universal and existential operators.

[26] Kaufmann [1], p. 10.
[27] Weyl [1], p. 19.

We shall suppose, that no sentences occur in language L with finitely restricted operators or with free variables. As mentioned before, the former ones have the same character as molecular sentences; the latter ones have the same character as sentences with universal operators. For the sake of simplicity we will consider in the following only operators of the lowest type, i.e. those with individual variables, not with predicate- or functor-variables. The operators of the lowest type are the most important ones in physics and generally in science; and all fundamental problems of meaning, confirmation and testing discussed in present philosophy already arise in connection with these operators. – Accordingly the term 'operator (in L)' is to be understood in the following as 'operator (not finitely restricted) with an individual variable'.

The purpose of the following considerations is to enable us to decide whether or not we will admit the application of operators in L and, if so, to what extent. In the following, 'M_1', 'M_2', etc. are taken as molecular predicates. Any molecular sentence can be transformed into (i.e. is equipollent to) a full sentence of a molecular predicate defined in a suitable way.

If we at all admit operators in L we may allow beside generalized sentences of the simplest form, such as '$(x)M(x)$' and '$(\exists x)M(x)$', also those with a more complicated form, as e.g. '$(\exists x)(y)M_1(x, y)$' or '$(x)(\exists y)(z)M_2(x, y, z)$'. The last example corresponds to the English sentence (of L'): "For every point x there exists a point y such that for every point z $M_2(x, y, z)$".

In Theorem 3, § 6, we stated a certain relation between '$(x)P_1(x)$' (S_1) and the full sentences of 'P_1'. Now the same relation subsists between '$(x)(y)P_2(x, y)$' (S_2) and the full sentences of 'P_2' because '$P_2(a, b)$' is a consequence of '$(y)P_2(a, y)$'; this last is a consequence of S_2, so that '$P_2(a, b)$' is itself a consequence of S_2, although S_2 is not a consequence of any finite class of full sentences of 'P_2'. Furthermore, the relation which we stated in Theorem 4 (§ 6) between '$(\exists x)P_1(x)$' (S_3) and the full sentences of 'P_1' also subsists between '$(\exists x)(\exists y)P_2(x, y)$' ($S_4$) and the full sentences of 'P_2'; for S_4 is a consequence of '$(\exists y)P_2(a, y)$', which is a consequence of '$P_2(a, b)$', so that S_4 is a consequence of '$P_2(a, b)$', although $\sim S_4$ is not a consequence of any finite class

of negations of full sentences of 'P_2'. Thus we see that for the question of confirmation a series of several operators of the same kind – that is to say all of them universal or all of them existential – has the same character as one operator of that kind.

First we will deal with only such generalized sentences of L as contain molecular predicates only. A sentence of this kind is constructed out of molecular predicates with the help of connections and operators. As is well-known such a sentence can be transformed into the so-called normal form[28] consisting of an operand which does not contain operators and is preceded by a series of operators without negation symbols. With the help of a molecular predicate defined in a suitable way we may transform the operand into 'M(x, . .)'. We next divide the series of operators of such a sentence S into sub-series each containing one or several operators of the same kind, that is to say, all of them universal or all of them existential; we call these sub-series the operator sets of S. Finally, we classify the sentences of the form described in the following way. The class of those sentences which have n operator-sets is called U_n, if the first operator is a universal one, and E_n, if the first operator is an existential one. The class U_0 is the same as E_0; it is the class of the molecular sentences. Instead of "a sentence of the form U_n" we shall write shortly "a U_n"; and analogously "an E_n". A U_1 has one or more universal operators only, an E_1 one or more existential operators. To U_2 belong the sentences of the form '$(x)(\exists\, y)M(x, y)$', but likewise '$(x_1)(x_2)(\exists\, y_1)(\exists\, y_2)(\exists\, y_3)M(x_1,x_2,y_1,y_2,y_3)$' etc., and generally every sentence consisting of a set of universal operators succeeded by a set of existential operators and by a molecular operand. To U_3 belongs every sentence constructed in the following way: first a set of universal operators, then a set of existential operators, then a set of universal operators, and finally a molecular operand.

Theorem 17. If S is a U_{n+1}, the confirmation of S is incompletely reducible to that of certain E_n, and the confirmation of $\sim$ S is completely reducible to that of each among certain U_n.

Proof. For n = 0, this follows easily from Theorem 3 (§ 6).

[28] Comp. Hilbert [1] p. 63; Carnap [4b] §34b, RR 9.

For $n > 0$, let S be '$(x_1)(\exists x_2)(x_3) \ldots x_{n+1})M(x_1, \ldots x_{n+1})$'. We define 'P' by '$P(x_1) \equiv (\exists x_2)(x_3) \ldots x_{n+1})M(x_1, \ldots x_{+1})$'. Then S can be transformed into '$(x_1)P(x_1)$'. Therefore, according to Theorem 3 (§ 6), the confirmation of S is incompletely reducible to that of the full sentences of 'P'; and the confirmation of $\sim$ S is completely reducible to anyone of their negations. Now a full sentence of 'P', say 'P(a)', can be transformed into '$(\exists x_2)(x_3) \ldots x_{n+1})M(a, x_2, \ldots x_{n+1})$' and is therefore an E_n. '$\sim P(a)$' can be transformed into '$(x_2)(\exists x_3) \ldots x_{n+1})$ $[\sim M(a, x_2, \ldots x_{n+1})]$' and is therefore a U_n.

Theorem 18. If S is an E_{n+1}, the confirmation of S is completely reducible to that of each among certain U_n, and the confirmation of $\sim$ S is incompletely reducible to that of certain E_n.

Proof. For $n = 0$, this follows easily from Theorem 4 (§ 6). For $n > 0$, let S be '$(\exists x_1)(x_2)(\exists x_3) \ldots x_{n+1})M(x_1, \ldots x_{n+1})$'. We define 'P' by '$P(x_1) \equiv (x_2)(\exists x_3) \ldots x_{n+1})M(x_1, \ldots x_{n+1})$'. Then S can be transformed into '$(\exists x_1)P(x_1)$'. Therefore, according to Theorem 4 (§ 6), the confirmation of S is completely reducible to that of any full sentence of 'P'; and the confirmation of $\sim$ S is incompletely reducible to that of the negations of the full sentences of 'P'. A full sentence 'P(a)' can be transformed into '$(x_2)(\exists x_3) \ldots x_{n+1})M(a, x_2, \ldots x_{n+1})$' and is therefore a U_n. '$\sim P(a)$' can be transformed into '$(\exists x_2)(x_3) \ldots x_{n+1})$ $[\sim M(a, x_2, \ldots x_{n+1})]$' and is therefore an E_n.

Theorem 19. If the primitive predicates of a language are observable – as they are e.g. in L – and if S is a U_1, i.e. of the form '$(x)M(x)$', the following is true. a. S is incompletely confirmable and $\sim$ S completely confirmable. b. If only test chains are admitted – as e.g. in L in the case of Decision 3a – S is incompletely testable and $\sim$ S completely testable. – This follows from Theorem 3 (§ 6) and Theorem 16 (§ 22).

Theorem 20. If the primitive predicates of a language are observable – as they are e.g. in L – and if S is an E_1, i.e. of the form '$(\exists x)M(x)$', the following is true. a. S is completely confirmable and $\sim$ S incompletely confirmable. b. If only test chains are admitted – as e.g. in L in the case of Decision 3a – S is completely testable and $\sim$ S incompletely testable. – This follows from Theorem 4 (§ 6) and Theorem 16 (§ 22).

The Theorems 19 and 20 correspond to the customary but not quite correct formulation: "a universal sentence is not verifiable but falsifiable; an existential sentence is verifiable but not falsifiable."

Theorem 21. If the primitive predicates of a language are observable – as they are e.g. in L – and if S is a U_n or an E_n with $n > 1$, thus containing at least one universal operator and simultaneously at least one existential operator, the following is true. a. Both S and $\sim$ S are incompletely confirmable, and hence S is bilaterally confirmable. b. If only test chains are admitted both S and $\sim$ S are incompletely testable, and hence S is bilaterally testable. – This follows from Theorems 17 and 18.

Thus we have seen that all generalized sentences of L of the forms described before are confirmable, and, in the case of Decision 3a, testable. The E_1 and the negations of U_1 are completely confirmable (or completely testable, respectively); all the other generalized sentences – provided they are essentially generalized – are only incompletely confirmable (or incompletely testable, respectively). No essentially generalized sentence is bilaterally completely confirmable or bilaterally completely testable.

25. The Scale of Languages

This being the case, how shall we decide about admitting of generalized sentences in the language L? This is the most critical question. In regard to it there are fundamental differences among philosophers, which are very sharply discussed. There is an infinite number of possible answers, i.e. of possible choices concerning the limitation of language. Among the possible language-forms we may choose the chief ones and order them in a series with regard to the highest degree of complexity admitted in them. But how may we determine this degree? It is natural to assume, if $m > n$, that a U_m is more complicated than a U_n, and an E_m as more so than an E_n. But how are we to decide the order of U_n with respect to E_n? We may do so by establishing the convention to take U_n as simpler than E_n. This convention is practically justified by the fact that some philosophers admit

U_1 but not E_1, or U_2 but not E_2; the attempt to give theoretical reasons for this convention has been made by Popper, as we shall see. Thus we obtain a progression of languages L_0, L_1, etc., starting with the molecular language L_0 and going on to languages of greater and greater extentions. Every language in the following table contains the sentences of the previous languages and, in addition, the sentences of the class given in the second column. After this endless series we may put the language L_∞ which is to contain all the sentences of the languages of the series L_0, L_1 . . . L_n, . . . (with finite n) but no others.

Language	Sentences of maximal complexity admitted in L_n	
	Class	Example
L_0	U_0, E_0 (both molecular)	$M_1(a)$
L_1	U_1	$(x)M_1(x)$
L_2	E_1	$(\exists x)M_1(x)$
L_3	U_2	$(x)(\exists y)M_2(x, y)$
L_4	E_2	$(\exists x)(y)M_2(x, y)$
L_5	U_3	$(x)(\exists y)(z)M_3(x, y, z)$
L_6	E_3	$(\exists x)(y)(\exists z)M_3(x, y, z)$
.	.	.
.	.	.
.	.	.
L_∞	no maximal complexity; sentences of any such class with any number of operator sets are admitted.	

Note on L_0, molecular language. We have considered above some examples of philosophers who propose or require L_0, that is, who demand the limitation to molecular sentences. From our last considerations it is clear that to accept the requirement of complete confirmability or that of complete testability means to exclude generalized sentences and hence to state L_0. The step of dropping that requirement and choosing one of the wider languages instead of L_0 is a decisive one. One of the chief reasons in favour of this decision is the fact, that both methods of interpreting physical laws in the case of L_0 which we mentioned above

(§ 23) are not very convenient for practical use and, above all, are not in close conformity with the actual method adopted by physicists. For in the first place, in actual practice laws are not dealt with as reports; and secondly, they are connected with one another or with singular sentences in a form of a disjunction or conjunction or implication or equivalence, etc.; in other words: they are manipulated like sentences, not like rules. (These reason are not proofs for an assertion, but motives for a decision.)

I believe that *Morris*[29] is right in saying that by the step described, i.e. the adoption of a generalized language which is able to express physical laws in a satisfactory way, we ("logical positivists") come to a closer agreement with pragmatism. Morris[30] considers the two movements as complementary in their views, and as convergent in the directions of their present development.

Note on L_1. We may take *Popper's*[31] *principle of falsifiability* as an example of the choice of this language. Popper is however very cautious in the formulation of his limiting principle ("Abgrenzungskriterium"); he does not call the sentences E_1 meaningless, but only non-empirical and metaphysical. (Perhaps he wishes to exclude existential sentences and other metaphysical sentences not from the language altogether, but only from the language of empirical science.) At first sight, universal and existential sentences seem to be coördinate with each other. In pure logic there is indeed a complete symmetry between them (principle of duality), but in epistemology, i.e. in applied logic considered from the point of view of confirmation and testing, there is difference[32] which has often been noticed. – Also some intuitionists object more to existential than to universal sentences, and sometimes only to the former ones. Therefore they may perhaps be taken as supporters of L_1. – I[33] have stated a language

[29] Morris [1] p. 6.

[30] l.c., p. 1.

[31] Popper [1], p. 12, 33.

[32] Popper ([1] Ch. II and IV) especially has emphasized the fact that for scientific testing falsifiability is more important than verifiability, and therefore (in our terminology:) sentences whose negations are completely confirmable are preferable to those whose negations are only incompletely confirmable though they are themselves completely confirmable, and hence U_1 preferable to E_1.

[33] Carnap [4], Language I.

which contains U_1 (with free variables, not with operators) but not E_1 and therefore may also be taken as an example of L_1; but this language has not been proposed as the language of science.

Note on L_3. While Popper in theory states the principle of falsifiability and in consequence takes the language-form L_1, in practice he seems to me to take the more liberal form L_3. He shows that probability-sentences are sentences of the form U_2 which he calls existential hypotheses ("Es-gibt-Hypothesen"[34]). He admits that probability-sentences are essential for physics, and therefore he includes them into the language of physics, which thus seem to have the form L_3. The way in which he tries to show that the admission of existential hypotheses is compatible with his requirement of falsifiability, is less important for our present consideration. He admits that they are neither falsifiable nor verifiable[35] – in our terminology: neither their negations nor they themselves are completely confirmable – but he tries to show that according to certain methodological rules they are manipulated like falsifiable sentences and actually are sometimes falsified.[36]

Note on L_∞. I am at present inclined to accept this most liberal form of language, including sentences with any number of operator-sets. If one sees, e.g. from Popper's explanations, how convenient and even essential the sentences U_2 are for physics, and if in consequence one decides to admit this form, then it seems rather arbitrary to limit the number of operator-sets to two or any fixed higher number and not to admit more complicated forms. It is true that the greater the number of operator-sets in a sentence S is, the greater is the distance of S from the empirical basis, i.e. from the atomic sentences, and hence the more indirect and incomplete is the possibility of confirming or testing S and $\sim$ S. But there is no number of operator-sets for which the connection with the empirical basis would completely vanish. If operators once are admitted and thereby the requirement of complete confirmability or complete testability is dropped, there

[34] Popper [1], p. 135.
[35] l.c., p. 134.
[36] l.c., p. 140, 144.

seems to me to be no natural limit at any finite number of operator-sets.

After anyone of the languages $L_0, L_1 \ldots L_\infty$ is chosen we may decide between Decision 3a and 3b (§ 21). In the case of Decision 3a all introductive chains are test chains and hence all predicates and all sentences of the language are *testable*. A language L_n restricted in this way, may be designated by 'L_n^t'. Thus we have a second series of languages: $L_0^t, L_1^t, L_2^t, \ldots L^t{}_\infty$.

I C 2: Generalized predicates. If we have a language in which operators are admitted then we may also admit them in definitions, i.e. state generalized definitions and general introductive chains containing such definitions.

We have considered so far only such generalized sentences as have a molecular operand. We did this for the sake of simplicity, because the definition of the single languages of the series L_0, L_1, etc. can be stated more easily in this case. But if we come to language L_∞ in which the use of operators is not limited then for this language we may also admit the occurrence of any number of generalized predicates in the operand.

26. Incompletely Confirmable Hypotheses in Physics

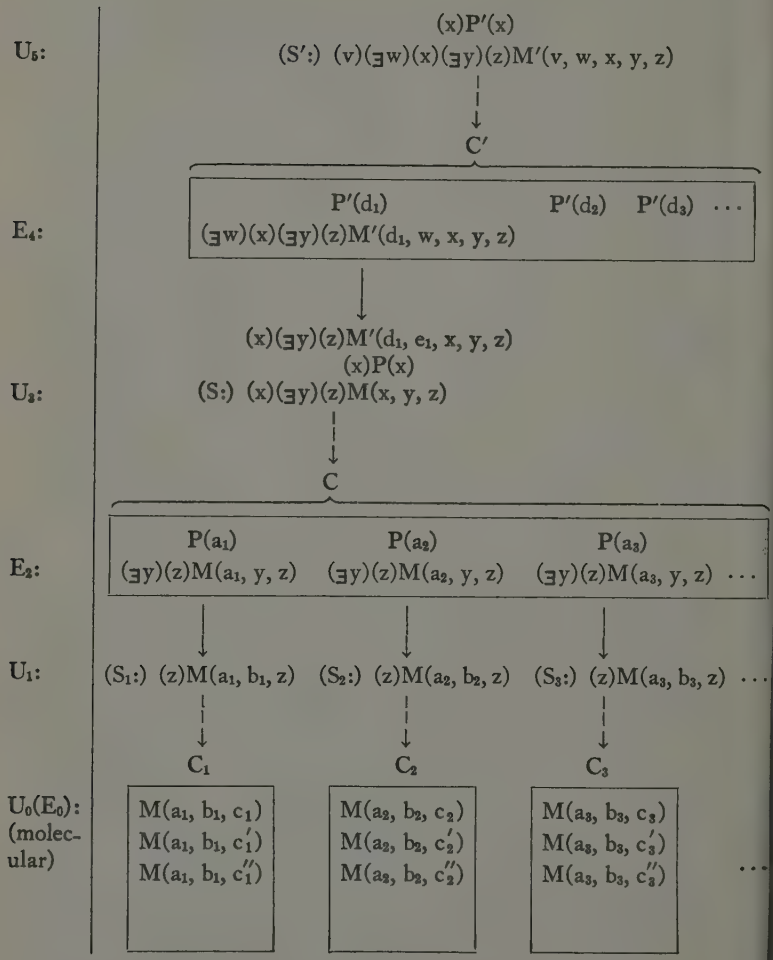
Now let us consider under what circumstances a physicist might find it necessary or desirable to state an hypothesis in a generalized form. Let us begin with one operator. The full sentences of a molecular predicate 'M_1' (i.e. '$M_1(a)$', etc.) are bilaterally completely confirmable. Suppose some of them are confirmed by observations, but not the negation of any of them so far. This fact may suggest to the physicist the sentence '$(x)M_1(x)$' of U_1 as a physical law to be adopted, i.e. a hypothesis whose negation is completely confirmable and which leads to completely confirmable predictions as consequences of it (e.g. '$M_1(b)$' etc.). If more and more such predictions are confirmed by subsequent observations, but not the negation of any of them, we may say that the hypothesis, though never confirmed completely, is confirmed in a higher and higher degree.

Considerations of this kind are very common; they are often used in order to explain that the admission of not completely con-

firmable ("unverifiable") universal hypotheses does not infringe the principle of empiricism. Such considerations are, I think, agreed to by all philosophers except those who demand complete confirmability ("verifiability") and thereby the limitation to a molecular language.

Now it seems to me that a completely analogous consideration applies to sentences with any number of operator sets, i.e. to sentences of U_n or E_n for any n. The following diagram may serve as an example. A *broken* arrow running from a sentence S to a class C of sentences indicates that the confirmation of S is *incompletely* reducible to that of C. S is in this case a universal sentence and C the class of its instances; each sentence of C is therefore a consequence of S, but S is not a consequence of any finite sub-class of C. A *solid* arrow running from S_1 to S_2 indicates that the confirmation of S_1 is *completely* reducible to that of S_2. In this case, S_1 is an existential sentence and a consequence of S_2. The relation of reducibility of confirmation as indicated in the diagram is in accordance with Theorems 17 and 18 (§ 24), but, for these cases, can easily be seen by glancing at the sentences. At the left side are indicated the classes to which the sentences belong.

Let us start at the bottom of the diagram. The sentences of C_1 are molecular, and hence bilaterally completely testable. Let us suppose that a physicist confirms by his observations a good many of the sentences of C_1 without finding a confirmation for the negation of any sentence of C_1. According to the customary procedure described above these experiences will suggest to him the adoption of S_1 as a well-confirmed hypothesis, which, by further confirmation of more and more sentences of C_1, may acquire an even higher degree of confirmation. Let us suppose that likewise the sentences of C_2 are confirmed by observations, further those of C_3, etc. Then the physicist will state S_2, S_3 etc. as well-confirmed hypotheses. If now sentences of the form E_2 are admitted in L, then the first sentence of C is a sentence of L, is also a consequence of S_1 and is therefore confirmed to the same degree as S_1. In order to make feasible the formulation of this well-confirmed hypothesis the physicist will be inclined to admit


U_5:
(x)P′(x)
(S′:) (v)(∃w)(x)(∃y)(z)M′(v, w, x, y, z)
C′
E_4:
P′(d1) P′(d2) P′(d3) ...
(∃w)(x)(∃y)(z)M′(d1, w, x, y, z)
(x)(∃y)(z)M′(d1, e1, x, y, z)
U_3:
(x)P(x)
(S:) (x)(∃y)(z)M(x, y, z)
C
E_2:
P(a1) P(a2) P(a3)
(∃y)(z)M(a1, y, z) (∃y)(z)M(a2, y, z) (∃y)(z)M(a3, y, z) ...
U_1:
(S1:) (z)M(a1, b1, z) (S2:) (z)M(a2, b2, z) (S3:) (z)M(a3, b3, z) ...
C1 C2 C3
$U_0(E_0)$: (molecular)
M(a1, b1, c1) M(a1, b1, c1′) M(a1, b1, c1″)
M(a2, b2, c2) M(a2, b2, c2′) M(a2, b2, c2″)
M(a3, b3, c3) M(a3, b3, c3′) M(a3, b3, c3″) ...

the sentences of E_2 in L. If he does so he can go one step further. He will adopt the second sentence of C as a consequence of the stated hypothesis S_2, the third one as a consequence of S_3, etc. If now the sentences of a sufficient number of classes of the series C_1, C_2, etc. are confirmed by observations, the corresponding number of sentences of the series S_1, S_2, etc. and likewise of sentences of C will be stated as well-confirmed hypotheses. If we define 'P' by '$P(x) \equiv (\exists\, y)(z)M(x, y, z)$', we may abbreviate the sentences of C by '$P(a_1)$', '$P(a_2)$', etc. The fact that these sentences are well-confirmed hypotheses will suggest to the physicist the sentence '$(x)P(x)$', that is S, as a hypothesis to be adopted provided he admits at all sentences of the form U_3 in L. The statement of S as confirmed by C is quite analogous to that of S_1 as confirmed by C_1. If somebody asserted that S – belonging to U_3 – is meaningless while the sentences of C – belonging to E_2 – are meaningful, he would thereby assert that it is meaningless to assume hypothetically that a certain condition which we have already assumed to subsist at several points a_1, a_2, a_3, etc. subsists at every point. Thus no reason is to be seen for prohibiting sentences of U_3, if sentences of E_3 are admitted.

This same procedure can be continued to higher and higher levels. Suppose that in the definition of 'M' two individual constants occur, say 'd_1' and 'e_1'; then we may write S in the form '$(x)(\exists\, y)(z)M'(d_1, e_1, x, y, z)$'. According to our previous supposition this is a hypothesis which is incompletely confirmed to a certain degree by our observations, namely by the sentences of C_1, C_2, etc. Then the first sentence of C', being a consequence of S, is confirmed to at least the same degree. If we define 'P'' by '$P'(v) \equiv (\exists\, w)(x)(\exists!\, y)(z)M'(v, w, x, y, z)$' we may abbreviate the first sentence of C' by '$P'(d_1)$'. Now let us suppose that analogous sentences for d_2, d_3, etc. are likewise found to be confirmed by our observations. Then by these sentences of C' (belonging to E_4) S' (belonging to U_6) is incompletely confirmed.

On the basis of these considerations it seems natural and convenient to make the following decisions.

Decision 5. Let S be a universal sentence (e.g. '$(x)Q(x)$') – which is being considered either for admission to or exclusion

from L – and C be the class of the corresponding full sentences ('$Q(a_1)$', '$Q(a_2)$', etc.). Then obviously the sentences of C are consequences of S, and the confirmation of S is incompletely reducible to that of C.

a. If the sentences of C are admitted in L we will admit the sentences of the form S, i.e. a class U_n for a certain n ($n > 0$).

b. If the sentences of C are stated as hypotheses with a sufficiently high degree of confirmation, we will admit S to be stated as a hypothesis with a certain degree of confirmation, if no other reasons are against this, e.g. the negation of one of the sentences of C being confirmed to a sufficiently high degree.

Decision 6. Let S be an existential sentence (e.g. '$(\exists x)Q(x)$') – which is being considered either for admission to or exclusion from L – and C be the class of the corresponding full sentences ('$Q(a_1)$', '$Q(a_2)$', etc.) Then obviously S is a consequence of every sentence of C, and hence the confirmation of S is completely reducible to that of C.

a. If the sentences of C are admitted in L we will admit the sentences of the form S, i.e. a class E_n for a certain n ($n > 0$).

b. If at least one sentence of C, say S′, is stated as a hypothesis with a sufficiently high degree of confirmation, we will admit S to be stated as a hypothesis with a certain degree of confirmation at least equal to that of S′.

The acceptance of Decisions 5 and 6 leads in the first place, as shown by the example explained before, to the admission of U_1, E_2, U_3, E_4, U_5, etc. in L; and it also leads to the admission of E_1, U_2, E_3, U_4, etc. Hence the result is the choice of a language L_∞ or, if Decision 3a is made, language L_∞^t.

As an objection to our proposal of language L_∞ the remark will perhaps be made that the statement of hypotheses of a high complexity, say U_{10} or E_{10}, will never be necessary or desirable in science, and that therefore we need not choose L_∞. Our reply is, that the proposal of L_∞ by no means requires the statement of hypotheses of such a kind; it simply proposes not to prohibit their statement *a priori* by the formative rules of the language. It seems convenient to give the scientist an open field for possible formulations of hypotheses. Which of these admitted possibili-

ties will actually be applied, must be learned from the further evolution of science, – it cannot be foreseen from general methodological considerations.

27. The Principle of Empiricism

It seems to me that it is preferable to formulate the principle of empiricism not in the form of an assertion – "all knowledge is empirical" or "all synthetic sentences that we can know are based on (or connected with) experiences" or the like – but rather in the form of a proposal or requirement. As empiricists, we require the language of science to be restricted in a certain way; we require that descriptive predicates and hence synthetic sentences are not to be admitted unless they have some connection with possible observations, a connection which has to be characterized in a suitable way. By such a formulation, it seems to me, greater clarity will be gained both for carrying on discussion between empiricists and anti-empiricists as well as for the reflections of empiricists.

We have seen that there are many different possibilities in framing an empiricist language. According to our previous considerations there are in the main four different requirements each of which may be taken as a possible formulation of empiricism; we will omit here the many intermediate positions which have been seen to consist in drawing a rather arbitrary boundary line.

RCT. Requirement of Complete Testability: "Every synthetic sentence must be completely testable". I.e. if any synthetic sentence S is given, we must know a method of testing for every descriptive predicate occurring in S so that we may determine for suitable points whether or not the predicate can be attributed to them; moreover, S must have such a form that at least certain sentences of this form can possibly be confirmed in the same degree as particular sentences about observable properties of things. This is the strongest of the four requirements. If we adopt it, we shall get a *testable molecular language* like L^{t}_{0}, i.e. a language restricted to molecular sentences and to test chains as the only introductive chains, in other words, to those reduction sentences whose first predicate is realizable.

RCC. Requirement of Complete Confirmability: "Every synthetic sentence must be completely confirmable." I.e. if any synthetic sentence S is given, there must be for every descriptive predicate occurring in S the possibility of our finding out for suitable points whether or not they have the property designated by the predicate in question; moreover, S must have a form such as is required in RCT, and hence be molecular. Thus the only difference between RCC and RCT concerns predicates. By RCC predicates are admitted which are introduced by the help of reduction sentences which are not test sentences. By the admission of the predicates of this kind the language is enlarged to a *confirmable molecular language* like L_0. The advantages of the admission of such predicates have been explained in §14. It seems however that there are not very many predicates of this kind in the language of science and hence that the practical difference between RCT and RCC is not very great. But the difference in the methodological character of L_0^t and L_0 may seem important to those who wish to state RCT.

RT. Requirement of Testability: "Every synthetic sentence must be testable." RT is more liberal than RCT, but in another direction than RCC. RCC and RT are incomparable inasmuch as each of them contains predicates not admitted in the other one. RT admits incompletely testable sentences—these are chiefly universal sentences to be confirmed incompletely by their instances—and thus leads to a *testable generalized language*, like L_∞^t. Here the new sentences in comparison with L_0^t are very many; among them are the laws of science in the form of unrestricted universal sentences. Therefore the difference of RCT and RT, i.e. of L_0^t and L_∞^t, is of great practical importance. The advantages of this comprehensive enlargement have been explained in §§ 25 and 26.

RC. Requirement of Confirmability: "Every synthetic sentence must be confirmable". Here both restrictions are dispensed with. Predicates which are confirmable but not testable are admitted; and generalized sentences are admitted. This simultaneous enlargement in both directions leads to a *confirmable generalized language* like L_∞. L_∞ contains not only L_0^t but also L_0 and L_∞^t as

proper sub-languages. RC is the most liberal of the four requirements. But it suffices to exclude all sentences of a non-empirical nature, e.g. those of transcendental metaphysics inasmuch as they are not confirmable, not even incompletely. Therefore it seems to me that RC suffices as a formulation of the principle of empiricism; in other words, if a scientist chooses any language fulfilling this requirement no objection can be raised against this choice from the point of view of empiricism. On the other hand, that does not mean that a scientist is not allowed to choose a more restricted language and to state one of the more restricting requirements for himself—though not for all scientists. There are no theoretical objections against these requirements, that is to say, objections condemning them as false or incorrect or meaningless or the like; but it seems to me that there are practical objections against them as being inconvenient for the purpose of science.

The following table shows the four requirements and their chief consequences.

Requirement	restriction to *molecular* sentences	restriction to *test* chains	language
RCT: complete testability	+	+	L_0^t
RCC: complete confirmability	+	−	L_0
RT: testability	−	+	L_∞^t
RC: confirmability	−	−	L_∞

28. Confirmability of Predictions

Let us consider the nature of a *prediction*, a sentence about a future event, from the point of view of empiricism, i.e. with respect to confirmation and testing. Modifying our previous symbolism, we will take 'c' as the name of a certain physical system, 'x' as a corresponding variable, 't' as the time-variable, 't_0' as a value of 't' designating a moment at which we have made observations about c, and 'd' as a constant designating a certain time interval, e.g. one day or one million years. Now let us consider the following sentences

(S) $(t)[P_1(c, t) \supset P_2(c, t + d)]$

in words: "For every instant t, if the system c has the state P_1 at the time t, then it has the state P_2 at the time $t + d$";

(S_1) $P_1(c, t_0)$

"The system c has the state P_1 at the time t_0 (of our observation)";

(S_2) $P_2(c, t_0 + d)$

"The system c will have the state P_2 at the time $t_0 + d$". Now let us make the following suppositions. There is a set C of laws about physical systems of that kind to which c belongs such that S can be derived from C; the predicates occurring in the laws of C, and among them 'P_1' and 'P_2', are completely testable; the laws of C have been tested very frequently and each tested instance had a positive result; S_1 is confirmed to a high degree by observations. From these suppositions it follows, that S_1 and S_2, having molecular form and containing only predicates which are completely testable, are themselves completely testable; that the laws of C are incompletely testable, but (incompletely) confirmed to a rather high degree; that S, being a consequence of C, is also confirmed to a rather high degree; that S_2, being a consequence of S and S_1, is also confirmed to a rather high degree. If we wait until the time $t_0 + d$ it may happen that we shall confirm S_2 by direct observations to a very high degree. But, as we have seen, a prediction like S_2 may have even at the present time a rather high degree of confirmation dependent upon the degree of confirmation of the laws used for the derivation of the prediction. The nature of a prediction like S_2 is, with respect to confirmation and testing, the same as that of a sentence S_3 about a past event not observed by ourselves, and the same as that of a sentence S_4 about a present event not directly observed by us, e.g. a process now going on in the interior of a machine, or a political event in China. S_3 and S_4 are, like S_2, derived from sentences based on our direct observations with the help of laws which are incom-

pletely confirmed to some degree or other by previous observations.[37]

To give an example, let c be the planetary system, C the set of the differential equations of celestial mechanics from which S may be derived by integration, S_1 describing the present constellation of c—the positions and the velocities of the bodies—and d the interval of one million years. Let '$P_3(t)$' mean: "There are no living beings in the world at the time t," and consider the following sentence.

$$(S_5) \qquad P_3(t_0 + d) \supset P_2(t_0 + d)$$

meaning that, if in a million years there will be no living beings in the world then at that time the constellation of the planetary system will be P_2 (i.e. that which is to be calculated from the present constellation with the help of the laws confirmed by past observations). S_5 may be taken as a convenient formulation of the following sentence discussed by *Lewis*[38] and *Schlick*:[39] "If all minds (or: living beings) should disappear from the universe, the stars would still go on in their courses". Both Lewis and Schlick assert that this sentence is not verifiable. This is true if 'verifiable' is interpreted as 'completely confirmable'. But the sentence is confirmable and even testable, though incompletely. We have no well-confirmed predictions about the existence or non-existence of organisms at the time $t_0 + d$; but the laws C of celestial mechanics are quite independent of this question. Therefore, irrespective of its first part, S_5 is confirmed to the same degree as its second part, i.e. as S_2, and hence, as C. Thus we see that an indirect and incomplete testing and confirmation of S_2—and thereby of S_5—is neither logically nor physically nor even practically impossible, but has been actually carried out by

[37] *Reichenbach* ([3], p. 153) asks what position the Vienna Circle has taken concerning the methodological nature of predictions and other sentences about events not observed, after it gave up its earlier view influenced by Wittgenstein (comp. §23). The view explained above is that which my friends—especially Neurath and Frank—and I have held since about 1931 (compare Frank [1], Neurath [3], Carnap [2a], p. 443, 464 f.; [2b], p. 55 f., 99 f.).

[38] Lewis [2], p. 143.

[39] Schlick [4], p. 367.

astronomers. Therefore I agree with the following conclusion of Schlick concerning the sentence mentioned above (though not with his reasoning): "We are as sure of it as of the best founded physical laws that science has discovered." The sentence in question is meaningful from the point of view of empiricism, i.e. it has to be admitted in an empiricist language, provided generalized sentences are admitted at all and complete confirmability is not required. The same is true for any sentence about past, present or future events, which refers to events other than those we have actually observed, provided it is sufficiently connected with such events by confirmable laws.—

The object of this essay is not to offer definitive solutions of problems treated. It aims rather to stimulate further investigation by supplying more exact definitions and formulations, and thereby to make it possible for others to state their different views more clearly for the purposes of fruitful discussion. Only in this way may we hope to develop convergent views and so approach the objective of *scientific empiricism* as a movement comprehending all related groups,—the development of an increasingly scientific philosophy.

BIBLIOGRAPHY

For the sake of shortness, the following *publications* will be quoted by the here given figures in square brackets.

* Appeared after the writing of this essay.

Ayer,* [1] Language, Truth and Logic, London 1936.

Bridgman, P. W. [1] The Logic of Modern Physics. New York 1927.

Carnap, R. [1] Der logische Aufbau der Welt. Berlin (now F. Meiner, Leipzig) 1928.

[2a] Die physikalische Sprache als Universalsprache der Wissenschaft. Erkenntnis 2, 1932.

[2b] (Translation:) The Unity of Science. Kegan Paul, London 1934.

[3] Ueber Protokollsätze. Erkenntnis 3, 1932.

[4a] Logische Syntax der Sprache. Springer, Wien 1934.

[4b] (Translation:) Logical Syntax of Language. Kegan Paul, London, (Harcourt, Brace, New York) 1936.

[5] Philosophy and Logical Syntax. Kegan Paul, London 1935.

[6] Formalwissenschaft und Realwissenschaft. Erkenntnis 5, 1935. (*Congress* [1]).

[7] Ueber ein Gültigkeitskriterium für die Sätze der klassischen Mathematik. Monatsh. Math. Phys. 42, 1935.

[8] Les Concepts Psychologiques et les Concepts Physiques sont-ils Foncièrement Différents? Revue de Synthèse 10, 1935.

[9] Wahrheit und Bewährung. In: *Congress* [3].
[10] Von der Erkenntnistheorie zur Wissenschaftslogik. In: *Congress* [3].
[11] Ueber die Einheitssprache der Wissenschaft. Logische Bemerkungen zur Enzyklopädie. In: *Congress* [3].
[12] Gibt es nicht-prüfbare Voraussetzungen der Wissenschaft? Scientia, 1936.

Congress [1] Einheit der Wissenschaft. Bericht über die Prager Vorkonferenz der Internationalen Kongresse für Einheit der Wissenschaft, Sept. 1934. Erkenntnis 5, Heft 1–3, 1935.
*[2] Erster Internationaler Kongress für Einheit der Wissenschaft (Congrès Internat. de Philos. Scientifique), Paris 1935. [Report of Sessions.] Erkenntnis 5, Heft 6, 1936.
*[3] Actes du Ier Congrès Internat. de Philos. Scientifique, Paris 1935. 8 fasc. Hermann & Cie, Paris 1936.

Ducasse, C. J. *[1] Verification, Verifiability and Meaningfulness. Journ. of Philos. 33, 1936.

Feigl, H. *[1] Sense and Nonsense in Scientific Realism. In: *Congress* [3].

Frank, Ph. [1] Das Kausalgesetz und seine Grenzen. Springer, Wien, 1932.

Hempel, C. G. [1] Beiträge zur logischen Analyse des Wahrheitsbegriffs. Diss. Berlin, 1934.
[2] Ueber den Gehalt von Wahrscheinlichkeitsaussagen. Erkenntnis 5, 1935.
[3] On the Logical Positivist's Theory of Truth. Analysis 2, 1935.
[4] Some Remarks on 'Facts' and Propositions. Analysis 2, 1935.

Hilbert, D. und Ackermann, W. [1] Grundzüge der theoritischen Logik. Springer, Berlin, 1928.

Kaufmann, F. [1] Das Unendliche in der Mathematik und seine Ausschaltung. Deuticke, Wien 1930.

Lewis, C. I., [1] with Langford, C. H. Symbolic Logic. The Century Co., New York 1932.
[2] Experience and Meaning. Philos. Review 43, 1934.

Morris, Ch. W. [1] Philosophy of Science and Science of Philosophy. Philos. of Sc. 2, 1935.
[2] The Concept of Meaning in Pragmatism and Logical Positivism. Proc. 8th Internat. Congr. Philos. (1934). Prague 1936.
[3] Semiotic and Scientific Empiricism. In: *Congress* [3].

Nagel, E. [1] Verifiability, Truth, and Verification. Journ. of Philos. 31, 1934.
*[2] Impressions and Appraisals of Analytic Philosophy in Europe. Journ. of Philos. 33, 1936.

Ness, A. *[1] Erkenntnis und wissenschaftliches Verhalten. Norske Vid.-Akad. II. Hist.-Fil. Kl., No. 1. Oslo 1936.

Neurath, O. [1] Physicalism. Monist 41, 1931.
[2] Physikalismus. Scientia 50, 1931.
[3] Soziologie im Physikalismus. Erkenntnis 2, 1931.
[4] Protokollsätze. Erkenntnis 3, 1932.
[5] Radikaler Physikalismus und "wirkliche Welt." Erkenntnis 4, 1934.
[6] Pseudorationalismus der Falsifikation. Erkenntnis 5, 1935.
*[7] Le Développement du Cercle du Vienne et l'Avenir de l'Empirisme Logique. Hermann, Paris 1935.

*[8] Einzelwissenschaften, Einheitswissenschaft, Pseudorationalismus. In: *Congress* [3].

Popper, K. [1] Logik der Forschung. Springer, Wien 1935.

*[2] Empirische Methode. In: *Congress* [3].

Ramsey, F. P. [1] General Propositions and Causality. 1929. Published posthumously in: The Foundations of Mathematics, and other Logical Essays, p. 237–255. Harcourt, Brace, New York 1931.

Reichenbach, H. [1] Wahrscheinlichkeitslehre. Sijthoff, Leyden 1935.

*[2] Ueber Induktion und Wahrscheinlichkeit. Erkenntnis 5, 1935.

*[3] Logistic Empiricism in Germany and the Present State of its Problems. Journ. of Philos. 33, 1936.

*[4] L'Empirisme Logistique et la Désaggrégation de l'Apriori. In: *Congress* [3].

Russell, B. [1] see *Whitehead*.

[2] Our Knowledge of the External World. Open Court, Chicago and London 1914.

Russell, L. J. [1] Communication and Verification. Proc. Arist. Soc., Suppl. Vol. 13, 1934.

Schlick, M. [1] Die Kausalität in der gegenwärtigen Physik. Naturwiss. 19, 1931.

[2] Ueber das Fundament der Erkenntnis. Erkenntnis 4, 1934.

[3] Facts and Propositions. Analysis 2, 1935.

[4] Meaning and Verification. Philos. Review 45, 1936.

Stace, W. T. *[1] Metaphysics and Meaning. Mind 44, 1935.

Stebbing, S. L. [1] Communication and Verification. Proc. Arist. Soc., Suppl. Vol. 13. 1934.

Tarski, A. *[1] Der Wahrheitsbegriff in den formalisierten Sprachen. Stud. Philos. 1, 1936.

Waismann, F. [1] Logische Analyse des Wahrscheinlichkeitsbegriffs. Erkenntnis 1, 1930.

Weyl, H. [1] Die heutige Erkenntnislage in der Mathematik. Symposion 1, 1925; also published separately.

Whitehead, A. N. and Russell, B. [1] Principia Mathematica. (1910–12) 2nd ed. Cambridge 1925–27.

Wittgenstein, L. [1] Tractatus Logico-Philosophicus. Harcourt, Brace, New York 1922.

The Mechanical versus the Mathematical Conception of Nature

BY

PHILIPP FRANK

(TRANSLATED BY PHILIP SHORR)

I. HOW IDEALISTS AND MATERIALISTS VIEW MODERN PHYSICS

WHEN science of the 20th century is spoken of in opposition to that of the 19th century, a particularly characteristic attribute is often cited: namely, that since the time of Galileo and Newton the task of science has been to explain everything mechanistically. By analogy the world was to be conceived as a great machine. But the theories of the 20th century, above all the relativity and quantum theories, caused a revolution in science. It is seen today that nature can be described and understood not 'mechanistically' but only through abstract mathematical formulas. The world is no longer a machine but a mathematical formula.

Great significance has also been attached to this revolution for the philosophical world-view. It was said: a mathematical formula is something purely mental. The world therefore can no longer be understood in a materialistic sense. Materialism must be superseded by idealism. The physics of the 20th century is a victory for the "spiritualist" or, as it is sometimes less clearly expressed, idealistic world-view.

This view-point has its representatives both among the idealists and materialists. The former rejoice at the unexpected aid they

have received for their world-view from the progress of science itself. The latter blame modern physics for abandoning the paths of progress begun by Galileo and Newton and for promoting the return to the dark views of the Middle Ages.

We will cite from the works of a few English and German authors to illustrate the point of view of the idealists, and from the works of Soviet authors, the fears of the materialists.

At the centennial of the British Association of Science in 1931 General J. C. Smuts, its president, characterized the science of our times as opposed to that of the last century in the following manner:

"There is the machine or mechanistic world-view dominant since the time of Galileo and Newton, and now, since the coming of Einstein, being replaced by the mathematician's conception of the universe as a symbolic structure of which no mechanical model is possible. . . .

Into this scheme (of the mechanistical conception) it has been difficult, if not impossible, to fit entities like life and mind. . . . The space-time relativity conception of the world has overcome the difficulty by destroying the old concept of matter and reducing it from a self-subsistent entity to a configuration of space-time, in other words, to a special organisation of the basic world-structure. If matter is essentially immaterial structure or organisation, it cannot fundamentally be so different from organisation or life, which is best envisaged as a principle of organisation, or from mind, which is an active organisator."

In his well known book, *The Mysterious Universe*, the famous physicist, Sir James Jeans, expresses similar views even more clearly. He says for example:

"The signal for the revolution was a short paper which *Einstein* published in June 1905. And with its publication, the study of the inner working of nature passed from the engineer-scientist to the mathematician. . . . The essential fact is simply that all pictures which science now draws of nature, and which alone seem capable of according with observational fact, are mathematical pictures . . . nature seems very conversant with the rules of pure mathematics, as our mathematicians have formulated them in

their studies, out of their own consciousness and without drawing to any appreciable extent on their experience of the outer world. . . . Our remote ancestors tried to interpret nature in terms of anthropomorphic concepts of their own creation and failed. The efforts of our nearer ancestors to interpret nature on engineering lines proved equally inadequate. Nature refused to accommodate herself to either of these man-made moulds. On the other hand, our efforts to interpret nature in terms of the concepts of pure mathematics have, so far, proved brilliantly successful. It would now seem to be beyond dispute that in some way nature is more closely allied to the concepts of pure mathematics than to those of biology or of engineering. . . . In any event, it can hardly be disputed that nature and our conscious mathematical minds work according to the same laws. She does not model her behavior, so to speak, on that forced on us by our whims and passions, or on that of our muscles and joints, but on that of our thinking minds. . . . The universe cannot admit of material representation, and the reason, I think, is that it has become a mere mental concept. . . . To my mind, the laws which nature obeys are less suggestive of those which nature obeys in its motion than of those which a musician obeys in writing a fugue, or a poet in composing a sonnet . . . the universe can be best pictured as consisting of pure thought, the thought of what, for want of a wider word, we must describe as a mathematical thinker."

We see the physicist and astronomer Jeans has made use of this turn from a 'mechanistic' to a 'mathematical' understanding of physics to favor an uncertain religiously-tinted metaphysics. When the professional German philosophers, on the other hand, deal with this revolution in science, we find them erecting a 'scientific' metaphysics on this foundation.

To illustrate this tendency we will quote a few passages from B. Bavink's *Science and Religion*. After the author has shown that modern wave mechanics introduces the significant wave function only as probability, he says:

"But a mathematical probability is not a physical reality like a temperature or field strength or what not. With this new interpretation the whole material notion of substance disappears in

our hands. What remains then of the plain hard sharp heavy matter! A certain probability depending on formal mathematical laws, that energy or impulse is observable in a certain world-point. . . .

The physicist of to-day has learned an enormous advance from the point of view of his world-view that his atoms or electrons or whatever they may be are no longer to be regarded as 'rigid blocks of reality', from which no path can be found into the mental and spiritual realm: he sees far more, that all these structures are forms in perpetual flux which are only of interest to him as regards their form. In this manner, every time materialism is superseded."

In this necessary mathematical conception of nature Bavink, like Jeans, sees the foundations of an idealistic philosophy of physics and hence of science as a whole. The only difference is that he (Bavink) uses somewhat more technical philosophical terms.

One task Bavink proposes for an idealistic philosophy of nature is the following:

"It must still be shown how matter may be derived from pure psycho-mental data of the world. It is notorious how this purpose of idealism has always been wrecked. It never succeeded even in deriving the nature of a hydrogen atom from such psychic data. What is new in the situation today is that such a proposal is no longer as absurd today as it was even 20 years ago. For that which was until now an insuperable obstacle to such a proposal,—the 'rigid blocks of reality' of the unfortunate atomism,—has been resolved into pure form; and a mathematical form is as such something mental; it belongs, as Plato already saw, directly in the realm of the *logos* which is behind all things. . . .

There it stands this hard, cold, sober world of matter and its atoms, the existence of which has been proven beyond doubt. It is impossible to pass it by. All idealists ought to realize that, and thus to put an end to such useless attempts. The material world will be ruled by the mind only then when we really understand it as a product of mental powers. It doesn't help us in the least merely to postulate it, as idealisms in the past have done.

The material world and its worshipers, the materialists, will only ridicule us saying: Here is a single atom, the simplest of all, the hydrogen atom. Here, show us what you can do! If you can show us how we can conceive this as a product of psychic powers (potencies) then we will believe you. It seems that idealism can actually pass this test today. I will not assert that it has already passed it, but it can not be doubted that it is very close to it and has good prospects of success."

Some professional philosophers look upon modern physics as a direct return to the anthropomorphic-animistic physics of the Middle Ages, as it was practiced by Aristotelian scholasticism. Thus Aloys Wenzl, Professor at the University of Munich, says in his essay, *The Metaphysics of Modern Physics*:

"And in this manner the human struggle for insight in the world will have described a circle, or more correctly a zig-zag line.

The examination of nature began with an anthropomorphic representation of the material world, in that souls were ascribed to material things, and the relations between them were viewed as expressions of psychic relationships of love and hate. The tendency to reification led farther and farther away from such early representations, freed physics more and more from such images and of necessity led ever more toward a mathematical examination of nature. For there are two possible ways of making the facts of experience and the laws of experience meaningful. They must either be treated according to psychological laws or associated with the ideal forms of mathematics. Modern physics has followed this second method as far as possible. But if more is desired; if assertions about their meaning are to be made, the mathematically expressed relationships must be explained. In this fashion a much higher plane, leading back to the original method is reached if not in physics, then in metaphysics."

If, then, we observe how a philosopher of the 20th century 'explains' the physics of his day, we will see that the 'idealistic' explanation is not so far removed from the 'spiritistic' or mystical. In the same essay, Wenzl says:

"Therewith it is clear that the concept of matter has changed completely. . . . Only the mathematical method itself actually

defines the sphere of the material world. But we can no longer associate an idea of something dead, with this material world. If we do wish to make an assertation about its essence, it is much sooner a world of elemental spirits which are bound in their relationships and their formation of wholes to certain rules of the spiritual realm, that can only be comprehended mathematically; or to put it in other words, it is a world of lower spirits whose reciprocal relationships can be expressed in a mathematical form. We don't know what kind of relationships this mathematical form signifies. But we know the form. Only the mathematical forms themselves or God might know their inner significance. A very alert metaphysician might explain them at best by analogy to known psychic relationships."

All these utterances show what great hopes the supporters of idealism had in the crisis in physics of the 19th and 20th century, and still have today. But just as some political systems adopt idealism as the foundation for their philosophy, so the supporters of Communism acknowledge materialism as a foundation for their world-view.

When we read the writings of the spiritual leaders of Communism, we see everywhere that the crisis in physics and its utilization as propaganda for idealism is viewed with concern and alarm.

We need only glance at Lenin's moving philosophical *chef d' oeuvre* of contemporary Communism, which was published in 1908 under the title: *Materialism and Empirio-criticism: Notes on a Reactionary Philosophy.*

The fifth chapter is entitled · "The Latest Revolution in Science and in Philosophical Idealism."

Lenin speaks of the changes made in the conception of matter by the energetic and electromagnetic theories. The physicists inclined toward idealism often formulate the results of these new conceptions as follows: 'The atom is dematerialized. Matter disappears.'

And the Russian philosopher Valentinov who is an acknowledged supporter of Mach draws the (following) inference with reference to a world-view:

"The assertion that a scientific explanation of the world is found

in the simple foundation of materialism, is now no more than a myth, and a foolish one at that."

Says Lenin, "there isn't the slightest doubt about the association of the new physics, or rather a certain school of the new physics, with the school of Mach and other types of a modern idealistic philosophy."

"The nature of the crisis in modern physics consists in the overthrow of old laws and fundamental principles. Objective reality outside of consciousness is rejected and materialism is replaced by idealism and agnosticism. Matter has disappeared. This is the way the basic and typical difficulty created by the crisis is expressed."

Even in the year 1922 when Lenin was already at the head of the Soviet Government of Russia, he was much concerned about the dangers for the materialism that might and did arise out of the crisis, and therewith form the foundations of Communism. At that time it was the relativity theory that played the most important part in the crisis in physics.

On December 3, 1922, Lenin wrote in the Journal, *Under the Banner of Marxism*:

"We must keep in mind that as a result of the revolution taking place in science today, reactionary philosophical schools and tendencies are likely to arise. Therefore, the journal, *Under the Banner of Marxism*, must be concerned about this revolution in modern science, otherwise, struggling materialism can be neither struggling nor materialism.

If the great majority of middle class intellegenzia stands behind Einstein, who is not taking an active part in the fight against materialism, then it holds not only for Einstein, but for most of the great scientists since the end of the 19th century."

We find the same fears and the same struggle, if we examine the latest text books that have been introduced in Soviet colleges for teaching materialism. Thus we find in the text books, *Dialectic Materialism*, (v. I pp. 111 ff.) published in 1934 under the editorship of M. Mitin of the Moscow Philosophical Institute of the Communist Academy:

"The attempts made to think of motion without matter and of

force without underlying substance are laying the foundation for idealism and clericalism. At the present time we see the furthering of these same idealistic tendencies. As a result of their association with Einstein's Theory of Relativity many are inclined to imagine motion without matter."

The misuse of modern physics as propaganda for idealism is very clearly described in the following passages. In the Russian materialistic literature since Lenin, it seems, the followers of Mach are cited as representatives of subjective idealism.

Thus on p. 55 *ibid.* it is said:

"In place of the old unchangeable atoms there has appeared a system of moving electrons. Therefore, say the 'Machians', matter has disappeared. But actually, more exact principles are replacing primitive physical laws. Yet the followers of Mach say: There is no objective knowledge. . . . The latest quantum mechanics strengthens the concept of causality, and makes corrections in the old concept. The Machians, however, declare, that causality has disappeared."

In the latest Russian materialistic literature there is a great deal of criticism of the new physical theories, particularly quantum mechanics, on account of their mathematical and formalistic nature. In effect it is much like that of the German and English idealists (e.g. Bavink and Jeans). Only the materialists are naturally critical about the replacement of mechanics by mathematics as an 'idealistic' trend. I will cite as evidence an article that appeared in a Russian popular science journal, *Krasnaya Nov.* In the article entitled "Chemistry and the Structure of Matter," the author Orlov says:

"Quantum mechanics is today still under the spell of the fetish, mathematics. This means that the method of quantum mechanics is of a formal mathematical nature. The mathematical pattern permits the building of a bridge that unites empirical facts furnished by spectroscopy with the conduct of electrons, atoms, and molecules. But up to now we have no physical explanation for the formulas of quantum mechanics. Instead of that, it is often proposed to abandon the search for an interpretation of physical laws and to replace physical representations with

abstract mathematical symbols. It is in this that the fetish, mathematics consists."

We see from all this with what determination they are continually referring to the philosophical importance of the physics of the 20th century. It is remarkable how often scientists with sympathies for idealism, philosophers with scientific inclinations but of a spiritualistic shade, and advocates of materialism as a tool of political progress agree with one another in so many essential points. But at this point we must put the following question: Is it really correct to see in the physics of the 20th century a transition from a 'mechanical' to a 'mathematical' explanation, from a materialistic palpable or intuitive perceptibility (Anschaulichkeit) to a mathematically abstract intellectual spirituality? (Geistigkeit)

Or has the rebirth of idealism entirely different sources, and is simply dragging along with it whatever material comes to hand?

II. PHYSICS OF THE 19TH CENTURY

The physics of Galileo and Newton which put an end to the animistic period was designated affectionately as 'mathematical'. At that time Newton wrote *The Mathematical Principles of Natural Philosophy*. Pronounced anti-materialistic tendency was ascribed to the work, because obvious palpable impacts of masses upon one another were replaced by a pure mathematical formula, the law of attraction.

The physics of Galileo and Newton came to be called 'mechanistic' only after the development of mechanics, which made more rapid progress than all other fields. It then became customary to use the treatment of mechanics as a model for every other field. The Newtonian mechanics was looked upon ever more as the standard type of a theory, so to speak, so that Helmholtz, for example said:

"To understand a phenomenon means nothing else than to reduce it to the Newtonian laws. Then the necessity of explanation has been satisfied in a palpable way."

It was completely forgotten that in Newton's day, his own theory was looked upon as an abstract mathematical formula,

which needed a mechanical or physical explanation to satisfy man's need for causality. Newton himself recognized it, and only declined to take part in it himself, when he made the now well known remark, "*Hypotheses non fingo*". But men like Huyghens and Leibniz never considered the Newtonian Theory as empirically satisfying, but always only as a mathematical formula. Already in the very beginning of the development of mechanistic physics, it was seen that it is not easy to establish the contradiction between a 'mechanistic' and a 'mathematical' theory.

In the course of time what great advantage was found in the mechanistic theories? At first it was uniformity (Einheitlichkeit) in all science, which, it was hoped, might be attained by reducing everything to mechanics, the most developed branch of science. But as that became ever more difficult and it was shown, for example, that an energetic or electro-magnetic representation was better adapted to bring all of physics or all of science under a unified theory, an entirely new advantage was discovered in mechanical theories. Namely, intuitive perceptibility (Anschaulichkeit). Whilst even in Descartes' time, the very 'mathematical' in the physical theories guaranteed their 'Anschaulichkeit' one began to find a contradiction between 'mathematical' and 'anschaulich', and to assert that only the reduction to mechanics guaranteed the Anschaulichkeit in a theory and that without Anschaulichkeit no real understanding was possible. It was claimed, for example, that Maxwell's field equations of electrodynamics are not 'anschaulich', if they are not illustrated with a mechanical model.

It may be said that there were two motives involved in the resistance to the abandonment of mechanism. They were:

A. There was no inclination to renounce the advantages of 'conceivability' which were ascribed to mechanistic theories only.

B. The achievements of Galilean-Newtonian epoch in mechanistic physics were defended, because it was feared that the abandonment of mechanistic explanations would lead to a return to medieval animistic authropomorphic science.

But if we speak again of this desire for Anschaulichkeit, we

ought to be clear as to the actual meaning of the term. The mechanical laws describe for us the grosser experiences of daily life, experiences in the use of tools such as firearms, lifting apparatus etc., as well as the most familiar experiences concerning the movements of the planets. We find it necessary to understand all other experiences as analogous to those that are most familiar.

The physics of the 19th century has already shown that this wish cannot be fulfilled. Electromagnetic phenomena cannot be reduced to the same mechanical laws that govern weapons, tools, etc.

Nevertheless the laws concerning electromagnetic phenomena may be considered in a broader sense as also 'anschaulich'. Only in this case we must consider how we can verify experimentally the accuracy of such physical laws as for example, Maxwell's field equations. We must derive from them an experimental result that is directly observable. But what kind of an observation is it? It is observed where the pointer of an ammeter or of some other measuring apparatus appears or how some colored spot is displaced. But they are precisely the same kind of observations that help to verify the laws of ordinary mechanics. In the end only gross mechanical events, which alone are observed, are derived from the equations of the electro-magnetic field. And that holds, as we will see, everywhere in physics even in that of the 20th century. In this the physical laws must always be 'Anschaulich', if they are to have any scientific meaning at all. For without 'Anschaulichkeit' in the western sense, they are not experimentally verifiable.

An explanation of fundamental equations using Anschauung on the contrary, is not a necessary demand of science. For the fundamental equations above cannot be submitted to any direct experimental verification. This 'non-Anschaulichkeit' of fundamental laws already existed in Maxwell's theory of the electromagnetic field. The 'electrical perturbation in a vacuum' was, for example, something entirely non-anschaulich, and yet a very essential element of this theory.

For a long time an effort was made to get a mechanical explanation of Maxwell's theory. For there was always the feeling that

without it, something essential was missing to the understanding of electromagnetic phenomena. Heinrich Hertz finally cut the Gordion knot, so to speak, when he said: "Maxwell's Theory is nothing else than Maxwell's equations. That is, the question is not whether these equations are anschaulich, i.e. can be interpreted mechanistically, but only whether anschaulich, interpretable conclusions can be derived from them by means of gross mechanical experiments."

Therewith was founded what we call today, the 'positivistic conception' of physics. Positivistic physics thus replaced mechanistic physics. And now the mechanistic explanation as a foundation could be abandoned, without at the same time renouncing the achievements of the Galilean-Newtonian epoch. With the aid of a positivistic conception of physics the rejection of mediaval animism was as complete as ever. The fear expressed in Bavink of the result of the abandonment of mechanistic physics became unjustified. In place of the mechanical model there was the mathematical formula with its experimentally verifiable results. In this sense, it may be said, that the positivistic conception had compensated the mechanistic interpretation with a mathematical one. Before anything was as yet known about relativity or quantum theories, before, therefore, even the 'rebirth of spiritualistic physics in the 20th century', Hertz, Mach, Duhem and others had already seen the essential point in every explanation of nature not in the mechanical model but rather in the construction of functional dependents.

The historical error is often made of connecting the struggle of Mach and Duhem for the positivistic physics with their aversion for atomism, so that a victory for atomism was considered a defeat for positivism.

In reality the champions of atomism, Maxwell and Boltzmann, were exactly of the same opinion concerning the general nature of a physical theory as Hertz and Mach. The difference in their views about the value of atomistic theories touched only upon the different estimate placed upon the suitability of these theories for the reproduction of physical phenomena. The opposition was a purely technical one, but by no means one of judgment or as we would rather say today, of scientific logic.

With the aid of a few statements from the writings of Maxwell and Boltzmann we can easily be convinced about what they thought concerning the structure of physical theories and their connection with experience.

In the introduction to his treatise, *On Faraday's Lines of Force*, J. C. Maxwell expressed himself quite clearly on these questions. Boltzmann says in his notes to the German translation of this work: "Maxwell's introduction proves that he was as much of a pioneer in the theory of knowledge as he was in theoretical physics. All the new ground in the theory of knowledge that was broken in the next forty years, is already clearly marked out in these few pages; indeed the very ideas are illustrated. Later epistemologists treated all this more fully, but also with greater bias. Then they set up rules for the further development of the theory after it had already developed and not before, as was the case here."

Thus Maxwell describes how he first came upon the mathematical formulation of the well known laws of electricity and of magnetism, and then upon the formulation of the new laws including the mathematical concepts so created:

"In order therefore to appreciate the requirements of the sciences the student must make himself familiar with a considerable body of most intricate mathematics, the mere retention of which in the memory materially interferes with further progress. The first process therefore in the effectual study of the science, must be one of a simplification and reduction of the resultants of previous investigation to a form in which the mind can grasp them. The results of this simplification may take the form of a purely mathematical formula or a physical hypothesis."

But Maxwell does not by any means think that there is an essential antithesis between a 'purely mathematical formula' and a 'physical hypothesis'. He judges both only according to their practical exchange value, and finds that each of the two has its advantages and disadvantages. Hence he looks for a kind of theory that is more general than either of them and that combines the advantages of both without the disadvantages. This more general sort of theory Maxwell finds in the physical analogy. It comprehends both the physical hypothesis, in which an analogy

is drawn between electromagnetic events and a mechanical model, and a mathematical formula which is only an analogy between a field of phenomena such as those of electricity and certain number relationships that are given by the electromagnetic field equations.

We will cite only a few characteristic propositions from Maxwell:

"In the first case (of a purely mathematical formula) we entirely lose sight of the phenomena to be explained and though we may trace out the consequences of given laws, we can never obtain more extended views of the connections of the subject. If, on the other hand we adopt a physical hypothesis, we see the phenomena only through a medium, and are liable to that blindness to facts and rashness in assumption which a partial explanation encourages. . . . In order to obtain physical ideas without adopting a physical theory we must make ourselves familiar with the existence of physical analogies. By a physical analogy I mean that partial similarity between the laws of one science and those of another which makes each of them illustrate the other. Thus all the mathematical sciences are founded on relations between physical laws and laws of numbers, so that the aim of exact science is to reduce the problems of nature to the determination of quantities by operation with numbers. Passing from the most universal of all analogies to a very partial one, we find the same resemblance in the mathematical form between two different phenomena giving rise to a physical theory of light."

In the introduction to his lectures on the theory of gases Boltzmann's view is brought out very clearly. In one place he says: "The question as to the fitness of the atomistic philosophy is naturally wholly untouched by the fact stressed by Kirchhoff that our theories about nature bear the same relation to it as symbols to the things symbolized, as letters to sounds or as musical notes to tones; and by the question whether it may not be expedient to consider our theories as pure descriptions so that we may always recall their relation to nature. Therefore the question is whether the pure differential equations or atomism will one day turn out the more complete descriptions of phenomena."

The opposition between Boltzmann and Mach therefore was

only in a difference of opinion as to whether the atomic theory constitutes a suitable medium for the description of experience, but by no means in a different understanding of the functions of a physical theory—as is often asserted by the mechanists and metaphysicists of the animistic-mystical school.

There is no doubt today that Mach's views concerning the worthlessness of atomism were often erroneous. But that was also the case on occasion with the physicists of the metaphysical school. The criticism made by Planck of Mach's conception of centrifugal forces need only be recalled. According to Mach these (forces) were to be determined by the relative motions of the bodies. This theory seemed absurd to Planck. Yet it soon after formed the basic idea of Einstein's gravitational theory.

We have seen that the transition from mechanistic to mathematical physics was the work of the positivistic doctrine in science and that it had nothing to do with the 20th century tendency toward idealism and metaphysics.

But it may well be, and that is often also declared, that positivistic physics itself has an element of spiritualism or idealism in it. The mathematical laws of which it consists, are brought into relation with sense perceptions, by means of assigned relationships, and directions for measuring magnitudes, represented by mathematical symbols. By means of such relationships, mathematical equations yield propositions about observations. It may therefore be said that positivistic physics consists of propositions about observations or perceptions. But then, so it is often expressed, it asserts something about the psychic. It has completely abandoned materialism and exactly like psychology has become a science of the mind. In fact, Ernst Mach built up all the sciences out of relationships of sensation.

Many, therefore, look upon positivistic physics as a variety subjective idealism, which declares that science can never assert anything about the actual world, but subjective sense impressions. In the above mentioned book, *Materialism and Empiriocriticism*, Lenin represented Mach's doctrine as a direct continuation of Bishop Berkeley's philosophy. He also blamed this doctrine for depreciating the actual world and for promoting the view that

behind the world of sense impressions about which alone science can assert something, lies the real world; but that is inaccessible even to science. According to this view of Lenin's, Mach seeks to strengthen the belief that the real world can only be explored through extra scientific or superscientific sources of knowledge such as metaphysics and religion.

This view of Mach's doctrine, however, is completely contradicted by Mach's actual inclinations. To be sure it must be admitted that his terminology might sometimes be a cause for such confusion. Also many of his disciples were in effect inclined toward idealism perhaps because of pressure to conform to the prevailing official philosophy according to which everything smacking of materialism was strictly taboo.

For Mach himself, the sensations with which he built up the entire science, were in no contradiction to the actual world that seemed inaccessible to reason. For him these sensations, which to avoid confusion he very often termed 'elements', were the building material he wished to use to create a unified system of science; this could be accomplished if one crossed from one field of science to another e.g. from physics to physiology.

For Mach the difference between the apparent and the real world could not at all be scientifically formulated. Yet the wholly gross mechanistic philosophy stood in no logical contradiction to Mach's conception. The bodies of daily experience with which mechanism constructed its world were also nothing else than a complex of sight and taste sensations. The question whether there really is a matter (a thing-in-itself) that is different from the sensation has no meaning for science since no experiment can be performed that might possibly settle the question. Every proposition about a gross material thing can also be expressed as about sensations. Thus the whole of mechanistic physics can be translated into the language of Mach.

Yet this language is much more comprehensive. Even though physics will not be reduced to mechanics, there is always the possibility of interpreting it as a system of propositions about elements of sensation. Mach admits a much more general building material than does mechanism. Mach's doctrine,

therefore, need not be looked upon as, 'overcoming of materialism' but rather as a generalization that includes the *mechanistic* materialism as a special case; which presumably is not in a position to embrace all of science. A logical contradiction exists only between a metaphysically conceived *Machism*, which is, then, subjective idealism, and a metaphysically conceived materialism which accepts matter only as having existence. But this doctrine was invented by idealists to refute materialism, and was, in any case, hardly representative of scientifically minded materialists.

The Neo-positivistic tendency represented by Carnap and the 'Vienna group', takes its point of departure from Mach, but so formulates its philosophy that it is quite impossible to confuse it with idealistic or solipsistic doctrines. For the new positivism the question of the existence of a real world behind our sensations is only a pseudo-problem. The raw material of science does not consist of elements of sensation or of bodies, but of propositions. Science is made up of a system of propositions in an inter-subjective language i.e. a language intelligible to all, of which 'physical language' is considered a type.

In the exact sciences the intersubjective language consists of mathematical formulae from which observational propositions can be derived with the aid of laws of logic and prescriptions for measurement.

We see then that it was positivism that repaired mechanistic science with mathematics. Yet it does not interpret either mechanics or mathematics metaphysically; nor does it find any sharp contradiction between the two types of physical theory, except the difference between a general mathematical and a special mechanical theory, as Maxwell already explained in the passage quoted above, from his work on *Faraday's Lines of Force.*

But the professional philosophers did not have to wait for the relativity and quantum theories of the 20th century to misconstrue the transition from mechanistic to mathematical theory. They succeeded in doing this with the physics of the 19th and earlier centuries. On more than one occasion they made use of the explanation of Mach's doctrine as subjective idealism. It

was even simpler than that. Wherever they saw something mathematical they tried to explain it as an ideal element that has found its home in materialistic physics.

Hermann Cohen, for example, the leader of the Neo-Kantian school of Marburg, says that mechanics has acquired a more spiritual character when such subtle mathematical concepts as the differential quotients had to be used to formulate its laws.

In the critical supplement to the seventh edition of Fr. A. Lange's *History of Materialism* (Vol. I pp. 504 ff) Hermann Cohen says: "The road of research leads straightway to idealism. Materialism is being destroyed at the roots of physical concepts, and it is mathematics that is leading in the emancipation which promises to be a lasting one. . . . Is there any other method of explaining reality, let alone establishing it, if not for the Newtonians' mass and force, or for the dynamicists' electrical mass? Is there any other method than the differential?"

Cohen shows however that something spiritual is to be derived from the differential.

But we have seen in part I that the idealistic interpretation of science, when taken seriously, is the first step in the return to an animistic anthropomorphic science of the medieval scholastics.

Positivism on the other hand particularly in its present form of a logically constructed empiricism, as it is represented by the Vienna Group, prevents the crisis in the mechanistic philosophy from spreading to the scientific world-view as a whole. It shows that the abandonment of mechanistic physics does not imply the need for a return to anthropomorphic physics of the Middle Ages. And right here something has happened which seems rather paradoxical. All those that advocate a return to pre-galilean science, whether it be under the name of 'idealism', 'holism' or 'organicism', or even under the name of 'race' and 'nation', fight with great ardor for the retention of the mechanistic philosophy in the field of physics, and condemn the positivistic physics as an aberration.

Evidence for this can be found by just turning the pages of "Journal for General Science" published in Germany since 1935. Its aim is the struggle against positivism in science and its

method of attack is in opposing 'German' science to 'French nationalism' and 'English empiricism'. In all this, the animistic-authropomorphic philosophy is half-willingly designated as specifically 'German'.

A few passages from K. Hillebrand's programmatic article, "Positivisim and Nature", may be cited here. They will make clear how an over estimation of mechanistic physics served in the fight against positivism and in behalf of authropomorphism.

Thus it is said: "Mechanism was a planned world picture constructed upon a principle; positivism accepts without choice every experience into the sum of its experiences. It accepts to be sure mechanistic explanations, as the equivalent of sense perceptions, but it denies as a matter of course the significance of every explanation, and gladly disavows them, for its aim is only description. . . . I therefore ask, isn't the principle advantage of positivism over mechanism tied up with as great a disadvantage? Is it really necessary or even pleasing to exchange a picturable concept of a mechanistic explanation for a pure mathematical formula, which transcends all perception? The break down of mechanism into positivism—very interesting as intellectual history—is an event, it seems to me, that is at present almost everywhere entirely misunderstood. And yet the principles of scientific method will never be determined without its clarification."

And now the author embarks upon an inspired glorification of mechanistic physics. He says:

"He who understands the running of a machine say a clock, based on the complete dependence of rigid bodies on spatial conceptions, he has genuine satisfying knowledge. It is the mistake of positivism that it is able to take this intention of Democritus as just one among other arbitrary hypotheses. It is also unfortunate for the development of positivism that it retains the objectivistic bias of mechanism and surrendered its only virtue, its pure, clear Anschaulichkeit."

As is almost always the case with those favoring an idealistic-organismic science, the enthusiasm for the mechanistic physics in its most obsolete form is coupled with a strong aversion for the

application of non-euclidean and multi-dimensional spaces. Hillebrand continues:

"Mechanism is euclidean science. Relativism of non-euclidean spaces, on the contrary, is the favorite child of positivism—A four dimensional space or a space with curved radii is just as logical for pure abstract thought as the euclidean; indeed the dissolution of space and time into mere abstract mathematical formulae seems to be a distinct gain to this other type of human beings. The overwhelming eternal advantages of euclidean space as over against these abstract arts ought not to be forgotten."

The reason for the glorification of 'mechanistic' physics by the advocates of organicism is that for their argument they need the application of a kind of physics that is as narrow as possible and therefore most unsuited for the more involved events.

Says Hillebrand: "It is evident from what has been said above that according to our way of thinking mechanism is far superior to positivistic empiricism—so long as it does not attempt to explain living matter. Besides, since Science has abdicated in this respect, there is nothing lost . . . the value therefore of exact scientific research is not attacked, in so far as it is restricted to 'dead' matter nature. The human mind possesses two sufficient types of knowledge: the explanatory and the understanding. Anschauung in the explanatory sense is the mechanistic explanation of nature, the representation of bodily form in an euclidean space-time relation going far beyond positivistic sense perception and yet conceivable or picturable in a narrower sense of sight and taste sensation. The understanding type of knowledge, is 'Anschauung' in a wider sense, perception not as sight sensation, but rather as a palpable human event that need not be 'sensible' but that is directed toward one of the senses."

The tendency is very clearly seen to allow mechanistic science to pass as the only 'explanatory' type of knowledge that is useful for exact science, so that it might be easier if necessary to introduce the so called 'understanding' type of knowledge in other fields.

As opposed to this, the new positivism represents the uniform character of science. It does not care to split human power to

know into 'mechanistic—explanatory' and 'understanding—perceiving' types. Positivistic logic of science of our time depicts the factual process of successful knowledge and scientific representation. Mechanistic explanation and understanding—sense-interpretation are accordingly only temporary, and, according to our understanding, more popular and more superficial types of scientific representation, but by no means particularly 'profound' types of knowledge.

III. PHYSICS OF THE 20TH CENTURY—THE THEORY OF RELATIVITY

The idea that the relativity and quantum theories are not 'Anschaulich' but only 'abstract mathematical' theories rests, above all, on the fact that in these theories are applied multidimensional spaces, non-euclidean curved spaces, probability amplitudes, wave functions in multi-dimensional spaces etc. instead of perceptible bodies of daily experience represented in three dimensional euclidean space—as was the case in classical physics. As in Part II the word 'Anschaulich' must be given two different meanings:

1. 'Anschaulichkeit' of the first principles of the theory.
2. 'Anschaulichkeit' of empirically verifiable conclusions.

Anschaulichkeit in the second sense is here too indispensable. Indeed it says in effect: assertions about gross bodies of daily experience must be derived from the theory. But no matter how refined a theory may be, or how complicated its methods of measuring, ultimately something quite gross is observed, the covering of a thread by a line, the similarity of two colored spots, etc. The observed conclusions from a theory are always mechanistic in a very tangible sense. They deal with events that can be represented according to the Newtonian mechanics of large bodies.

Anschaulichkeit in its first meaning demands that the first principles should be mechanically significant. But then they must assert something direct about the motion of large bodies. The laws of Newtonian mechanics do this, of course. But as we have seen in II, that does not hold any more for Maxwell's theory of the electromagnetic field. What is there mechanically per-

ceptible in the dielectrical disturbance of a vacuum? Often what is pretended as perceptible is an illusion. It is often thought, for example that an electron is perceptible because it is always imagined to be a little body, a corpuscle. But an electron is no body at all in the sense in which this concept is formed from the observations of large bodies described in classical physics. For example, it has no color; it has no definite form under illumination; it hasn't any of the velocity of independent masses.

As for the application of multi-dimensional spaces in the physics of the 20th century, the following must be considered: H. Hertz has already stated the first principles of the motion of a system of N points in the form that the representative point of the system describes a geodetic line. The Liouville proposition of statistical mechanics states that in the 6 N-dimensional phase space, the volume which is filled by the points of state, doesn't change in the course of time. It is said, indeed, that the employment of these spaces was only a form of speech that had no relation to the nature of these theories. But without this manner of speech these propositions would have been rendered clumsy and detailed. In the Newtonian mechanics the expressions 'force' and 'acceleration' are also only forms of speech. But an attempt might be made sometime to express the laws governing planetary motion without the help of these forms of speech. It will soon be observed that they became so detailed, that it will be just as well to say, there are no laws governing these motions.

In 1905 Einstein had first constructed the special relativity theory without the application of four dimensional space. When, Minkowski introduced these spaces, in 1908, the mathematicians were very sympathetic. But to the physicists it appeared as if they had obscured rather than clarified the meaning of the theory. If it is often said that the four-dimensional representation expresses the nature of the relativity theory more correctly than the three dimensional one, it is not very aptly stated. It is true that there is something arbitrary in the three dimensional description. A definite relational system must be applied every time. But actually no one of the systems is preferred. Nevertheless, if a definite system is applied, something is introduced

into the description of nature which corresponds to no observable phenomena. The proposition that only the four dimensional representation describes the nature of the phenomena correctly, is in this sense just as correct as the proposition: Only the Vector representation of the Newtonian equations gives a correct representation of Newtonian mechanics. In every representation of coördinates the direction of the axes is quite arbitrary and the application of the coördinates x, y, z introduces something into the phenomena which does not correspond to the observable. But taken logically, the equations in a coördinate and vector form express exactly the same thing. Both systems of expression are equipollent and alike in meaning. In Carnap's terminology of the 'logical syntax of language' they may be called L-equipollent since they can only be translated into one another by means of logical transformations.

The same holds true for the four-dimensional representation of the relativity theory. With the three dimensional system to which the Lorentz transformations also belong, it is analogous in meaning and interpretation.

It is quite correct that Minkowski expressed himself rather unfortunately in saying that from now on space and time are to be relegated to the empty shadows, and that only some kind of union of the two is to preserve reality. But this is only a pretty and fetching phrase for the mathematical fact that the Lorentz transformations have the form of the rotation of coördinates in four-dimensional space, and that the fundamental principles of mechanics and electrodynamics preserve their form in these transformations.

But with equal justice it could be said that only the 6 N-dimensional phase-space expresses correctly the Liouville proposition. Also it might be said that the 3 N-dimensional configuration space and the 3 N-dimensional impulse-space should be relegated to the empty shadows, and only their comprehensive form, the 6 N-dimensional phase-space, is to preserve reality.

What the philosophers say about the significance of the four-dimensional space for the relativity theory often rests on a misconception. They mistake lyricism for logic and a pretty phrase

for the discovery of a physical fact. They have said for instance that the relativity theory has 'proven the existence of four-dimensional space.' Or if they are not friendly toward Einstein, they said it 'was not proven'. Another inept assertion, found not only in philosophical literature but at times even in physics literature, runs as follows: In the Newtonian mechanics the motion of bodies is represented in euclidean space, and therefore 'Anschaulich'; in the general relativity theory—in non-euclidean curved space, and therefore 'non-perceptible'. But if the logical structure of the relativity theory is actually analyzed, it will be found that it is nonperceptible in the same sense as the Maxwell Theory of the electro-magnetic field. The fundamental equations themselves do not represent the motions of large bodies. But in both cases the derived magnitudes can be brought into union with observations of gross mechanical events; and thus observable phenomena can be derived from fundamental equations.

The value of the rectangular coördinates x, y, z, that are ordinarily employed in classical mechanics can be determined by constructing rigid measuring scales, that is by means of a simple device that can be described according to the mechanics of large bodies.

The coördinates X_1, X_2, X_3, X_3 that appear in the general relativity theory represent points in a four-dimensional continuum. Their values, however, can be determined by a simple process of measuring.

It is often called a 'Koordinatenmolluske', because the X_1, X_2, X_3, X_4 points are individualized in a continually changing medium without giving any measurable meaning to the values of the coordinates or their differences between two points.

But the general relativity theory includes a very definite even if somewhat complicated rule with the help of which the coördinates $X_1 \ldots X_4$, that appear in the equations of motion could be tied up with measureable results. The rules for such measuring processes are not, to be sure, everywhere the same, but depend on the physical conditions of the environment of the doubtful points, particularly on the gravitational field. In other words they depend on whether large bodies are found close by. If we

are given two points that are quite close to each other and their coördinate differences are given as dx_1, dx_2, dx_3, dx_4, then we will apply the rule with the help of which is established the connection between the numerical values of dx_k, which have next to no physical meaning, and the measuring of the distance between the points with the aid of a rigid measuring scale.

We will consider perhaps the simple case in which both points are differentiated only by the values of the X_1 coördinate, so that of dx_k only dx_1 is different from zero. We then take a very small rigid scale entirely unsupported—hence free in the gravitational field—and we measure with it the distance between any two points that have the coördinate differential dx_1. We then obtain the equation $ds = \gamma dx_1$ in which the factor γ may be changed from place to place and time to time, and therefore depends on the coördinates $X_1 \ldots X_4$. This magnitude which we designate as $\gamma = \sqrt{g_{11}}$, g_{11} is one of the ten gravitational potentials. Their differential quotients are connected with the gravitational forces according to the coördinates. If the gravitational potential g_{11}, and the other nine potentials g_{22}, g_{23}, $\ldots g_{44}$ that are similarly defined, are accepted as function of position, then with a definitely rigid measuring rod or a specially constructed exact clock, the general relation can be determined between the coördinate differentials $dx_1 \ldots dx_4$, and the spatial or temporal distance between both points.

This relation is given by Einstein's theory in the form

$$ds^2 = g_{11}\,dx_1^2 + 2g_{12}\,dx_1\,dx_2 + \cdots + g_{44}\,dx_4^2$$

This relation which assigns definite numbers *ds* to the differential magnitudes of $X_1 \ldots X_4$ that appear in the field equations, shows that the measuring device depends on the gravitational field, namely on the potentials $g_{11} \ldots g_{44}$. But since these potentials, according to the laws of gravitation, are dependent on the masses which generate the field, and since these gravitational laws were also constructed by Einstein in his general relativity theory, we may say that by measuring the masses generated in the field, we could construct the measuring devices by means of which definite numbers with physical significance can be assigned to the arbi-

trary coördinates $X_1 \ldots X_4$ which determine only the relative place on the "mollusk".

Exactly as the equations of the ordinary theory of the electro-magnetic field state the rules by means of which unintinted magnitudes can be measured according to their numerical value, such as the di-electrical displacement of a vacuum, similarly there reside in the Einstein equations of the gravitational field tied with the measuring methods, the devices for assigning numbers having a unique meaning to the unintintable coördinates of the 'mollusk system' and the gravitational potentials $g_{11} \ldots g_{44}$.

Every statement about the position of mass points, expressed in the 'mollusk'-coördinates, X_1, X_2, X_3, X_4, is absolutely equal in content (in the sense of Carnap's syntax, P-equipollent) to a statement about the results of measurements of large bodies. But it is not equal in sense perception because physical laws must also be employed to establish the relation.

The application of curved spaces is nothing else than the use of such mollusk-coördinates about which it is no longer presumed that they stand in so simple a relation to the measurable estimates in rigid measuring scales, as was the case in classical mechanics and the euclidian measuring systems. But the difference between the "physically intuitable" and the "mathematically abstract is not greater than in the classical theory of the electro-magnetic field despite the "curved" spaces in the Einstein gravitational theory. For one who has only studied Euclidean geometry the application of curved spaces may well be amazing; but from the point of view of scientific logic, it does not follow that the structure of the physical theory is changed. The general relativity theory is precisely as much and as little "mathematical" and precisely as much and as little "anschaulich" as the Maxwell field theory.

But after all is said, one thing is clear: if the laws concerning the swift motion of the atoms do not agree with those concerning the slow motion of large bodies, then they too can not be "mechanically-perceptible" in the old sense of the words. For to be "mechanically perceptible" means to be explainable in terms of the motion of large bodies, i.e. in terms of Newtonian equations of motion.

IV. PHYSICS OF THE 20TH CENTURY: THE QUANTUM THEORY

If it is said that the quantum theory is still less 'anschaulich' than the general relativity theory, then it can only mean that its propositions deviate even more from those made by Newtonian mechanics about the motion of large bodies.

Fundamentally, the logical structure of quantum mechanics cannot be any other than that of Maxwell's electromagnetic field theory. Its equations must represent relations between magnitudes, which can only be stated in estimates measured by rough mechanical devices. In order to comprehend it in detail, it is only necessary to analyze one case in which the quantum theory makes presumptions that are experimentally verifiable.

Let us consider electron diffraction flexion. Perhaps the simplest case—for esample—electron rays, from a cathode tube, are allowed to fall on a metal plate, and one observes the influence of the reflected rays on a photographic plate put in their path. It is noticed that the rays are reflected in accordance with the ordinary law of reflection; but only those rays are actually reflected that fall within definite angles, and only these rays produce a photochemical effect on the photographic plate, i.e. produces a blackening on the paper. These angles φ satisfy the equation $D \sin \varphi = n\lambda$, in which n represents any integer, D, any distance between two molecules in the metal plate and λ a constant, composed of mass m, velocity v and Planck's quantum of action h in the form $\lambda = \frac{h}{mv}$. This result can be derived theoretically, provided the space, in which radiation and photochemical effect proceed, is described by a place function—the Schrödinger "wave function". The assignment of its values at a point x, y, z, to measurable results is effected by means of the following relations: If a photographic plate is found in a place in space, then $(\varphi)^2$ i.e. the square of the absolute value of the wave function φ, is proportional to the intensity of the photochemical effect or the blackening of the paper. But if in a place of this space a Geiger point counter is set up, then it will produce electroscopic pulses that are connected with it, and the number of which in the unit of time is also proportional to the value of $(\varphi)^2$. A closer prediction within how short a time

these pulses would start, cannot be made from the knowledge of φ, but from the knowledge of a statistical average. There is therefore, an assignment of value of magnitude φ for wholly grossly observable events, such as the blackening of photographic papers and electroscopic pulses, that do not differ in the least from events observed in experiments of classical physics.

In the case of the blackening the value of φ is assigned to the result of a pauschal observation of the intensity of the blackening; with the point-counter it is given an average value obtained from many individual observations. The physical laws of the quantum theory consist, therefore, of relations between values of the kind described.

Relationships of this sort are known to classical physics. The Einstein law of the Brownian movement also predicts average values within a time-interval. If we examine very many small particles and measure each time the distance S which the particles cover in the time T, then the median value of all squares, S^2, comes to the sum total of 2Dt, where D represents the diffusion constant. Also "in a purely causal event", as in a freely falling body where the distance S traversed in the time t is represented by the equation $S = \frac{g}{2} t^2$, the formula can only be verified by the average value taken from many observations of the distance S in the time t. "Observational errors" are taken for granted. But this is reconstruction of the event. The test of a formula is always obtained from an average value gotten from many individual observations.

In the axiomatic construction of the quantum theory the relation between determinism and statistics plays a part entirely different from that in classical physics. According to the latter it should be possible to reduce the scattering of values of an observation, with the refinement of the experimental method, whereas according to present day physics, the scatterings of various observable magnitudes are so tied up with one another that not all can be made as small as one likes at the same time.

But in the verification of a concrete relation between observable magnitudes this difference hardly matters. Now as before, the problem is in verifying a relation between averages.

V. NATURE AND PURE MATHEMATICS

The proposition that nature is subject to the laws of "pure mathematics" can be given two interpretations:

1) The propositions of pure mathematics are confirmed by the experimental results of science. This assertion is meaningless. For the propositions of pure mathematics are pure, logical propositions. They are concerned therefore with what is said about the world and not at all with what happens in the world. The propositions of pure mathematics are, as it is also expressed, tautological, analytical, formal propositions. In any case, whatever may happen in nature, or whatever the results of the experiments may be, the propositions of pure mathematics remain unaltered. They are concerned with the description of events in various equivalent forms,—if we wish to express it this way. These diverse formulations of the same situation can be translated into one another whatever the observed events happen to be.

2) But the question whether nature can be interpreted in terms of pure mathematics can be understood in a different sense. In the physical theories of today, there are very often employed complicated mathematical concepts as the Riemannian curvature-tensor, infinite matrices, Hermite forms, etc. These concepts originated from considerations of pure mathematics, and only afterward became applicable to physical theories, such as the general relativity and quantum theories, that had not yet been discovered when these concepts were formulated.

Therefore, the world must be construed in its physical events too in terms of the laws of pure mathematics. For the concepts, created by the "pure" mathematicians without any knowledge of the physical laws have proved particularly fit for the formulation of these physical laws.

In his observations, Jeans draws an essential distinction between "pure" and "applied" mathematics. The applied mathematician, the engineer, for example, creates his mathematical concepts for the purpose of controlling concrete situations with their aid, say, in machines. But the fact that the world takes its course according to the formulas of "applied mathematics" is no proof of its mathematical construction. For these formulas are created

for the purpose of representing natural events. Pure mathematics, on the other hand, creates its concepts out of its own nature, from the instinct of play as it were. And now it seems that these concepts of pure mathematics can also be applied advantageously to physics.

Thus the general relativity theory can hardly be imagined without the aid of the concepts of the differential calculus of Ricci and Levi-Civita, or without the basic geometrical concepts of the Riemannian space theory. Quantum mechanics can hardly dispense with the Hamilton-Jacoby theory of contact transformations, and the transformations of the Hermite forms, etc. But all these concepts were created by "pure" mathematicians out of necessity. If they now prove to be useful, indeed indispensable to physics, then it is evident, according to Jeans, that nature is modeled after the ideas of a "pure" mathematician.

If a sound judgment about such ideas is really desired, it is necessary to consider very carefully how the mathematical concepts mentioned actually originated.

Let us begin therefore with a very simple example. Galileo described the motion of a freely falling body by asserting that the "acceleration" is constant. He created the concept of acceleration for the representation of this form of motion. But when Newton generalized Galileo's laws so as to include other types of motion it appeared that the generalization was possible, if, in describing important motions in nature, acceleration was no longer considered a constant, but rather dependent in a simple way on the position of the body in motion in relation to other bodies. The most important example of this is Newton's discovery that centripetal acceleration of the moon in its course around the earth is inversely proportional to the square of its distance.

It might also be said: it is remarkable that with the help of the concept of acceleration, which is the second differential quotient of the distances according to time, that is, a concept of "pure mathematics," it is possible to describe so well the movement of the planets. But it is very clearly seen in this example that acceleration has already been introduced to describe a known class of

natural events, namely Galileo's motions of falling bodies. In Jeans' sense, therefore, it is, to be sure, a concept of "applied" mathematics. But it appeared that it plays a part even in more general events than in motions for the benefit of which it was introduced.

Yet, it does not play a very important part. In Einstein's general gravitational theory, that is, in very fast motion in very strong gravitational fields, acceleration is no longer as important as it was in Newtonian mechanics. Here new mathematical concepts had to be employed.

"Potential" was introduced to describe the electrostatic field, that is, forces proceeding from masses at rest. It satisfied the Laplace equation or the condition for the disappearance of the curl of the electrical field of force. But if we have the field of electrical currents or other magnetic fields, then the curl of electrical field-strength does not disappear any more as in the case of the electrostatic field. But Maxwell has shown that the mathematical method discovered for the description of the electrostatic field, that is, the concept of the "curl" and that of the "Laplace operator" can also be applied to the description of more general fields. Only the curl is now no longer zero, but proportional to the variation in the magnetic field strength. Also the La-place expression of potential, instead of disappearing in the general field, is bound up with the temporal variation of potential by the wave equation. Maxwell himself clearly formulated this connection between mathematical method and physical theory. (See our citation from Maxwell's works.)

The general plan connecting existing mathematical concepts with new physical theories is always in the examples cited from the classical physics, the following:

1) A mathematical concept is constructed well adapted to a simple description of an already known physical fact. Example: Acceleration, curl of a vector field. With the help of these concepts the fact derived from it can be simply expressed: Constant of acceleration, disappearance of the curl.

2) The discovered mathematical concepts employed in the description of simple facts, often serve in the lucid description of

a more general class of natural events. 'Acceleration' helps in the construction of general Newtonian equations of motion; the curl of the field of force is very important in the Maxwell equations for general electromagnetic fields.

At times the known facts may be simply described by different mathematical concepts. But then not all of them are equally adapted for generalization. That system of concepts which is best adapted for the generalization of a large field of facts, is often spoken of as the 'most adequate representation' of the 'essence of the fact'.

Precisely the same considerations can be applied to the development of the relativity theory. Minkowski described very neatly Einstein's special relativity theory by demonstrating the symmetrical nature of the equations in space and time. The physical content of the theory was not in the least changed by this four dimensional mathematical representation. It was only, as some said, a 'formal game'. Many physicists at once thought that the meaning of the theory was obscured by such formalistic sleight-of-hand, or in any case made more difficult to understand. But Minkowski himself found, with the aid of his representation, the field equations for electromagnetic events within moving bodies of any di-electrical constant and magnetization number. As a result the mathematical concepts he created for representation of events in empty space proved to be of great help for the more general events in any bodies.

But Einstein himself soon departed from the Minkowski form of the special relativity theory. He generalized it further by introducing the absolute differential calculus of Ricci and Levi-Civitá, thus creating the mathematical basis for the representation of his general relativity theory. This generalization would hardly have been possible without the Minkowski four-dimensional representation of the special relativity theory. Its original three dimensional form would not have been appropriate for generalizing about motions that are not uniform and in a straight line.

For it is in the four-dimensional curved space that the basic concept of a geodetic line is to be found. Such a concept could not be derived from the description of uniform motion in a straight line in a three-dimensional space.

In any case the mathematical concepts necessary for this particular generalization were developed as stated in two stages. The concepts served as a point of departure for the description of motions of any deformable body, that is, general curvilinear coordinates. To find a junction to the customary system of measuring which makes use of rigid bodies, Einstein had to have the mathematical concepts for expressing the motion of rigid bodies in terms of motions of deformable bodies in curvilinear coördinates. It is well known that Einstein looked for such mathematical expressions. In his conversations with his mathematical colleagues, G. Pick in Prague and M. Grossmann in Zurich he soon found that the mathematicians had already developed such a system of concepts in the theory of transformation groups and their invariants (i.e. expressions that do not change while moving). In this manner Einstein was led to investigate the general differential calculus of Ricci and Levi-Civitá, which was a very appropriate mathematical method for a very neat representation of the ideas of Helmholtz, Riemann and Christoffel. These ideas aimed to represent the motion of a rigid body with the help of curvilinear coördinates.

The problem to be solved was: which variation expressed in curvilinear coördinates is the motion of a rigid body? Riemann and Christoffel answered this question in the form that certain expressions formed from the deformations must vanish in order to represent the motion of a rigid body. These expressions, which according to modern mathematical terminology form the components of a tensor (the Riemann-Christoffel curvature tensor) are the simplest mathematical elements for the investigation of physical processes in curvilinear coördinates, just as the ordinary curvature of a curve is the simplest element for the description of motion in curvilinear coördinates. For the simplest, force-free motion the curvature disappears and every effect of a force can be described most simply as an effect on the curvature.

According to the fundamental assumptions which led Einstein to his general theory of relativity, there could be no rigid bodies. He could not present the theory by means of orthogonal Cartesian coördinates. He had to turn to curvilinear coördinates. Since,

however, according to the Einstein postulates, bodies in a space free of masses, i.e. without a gravitational field, behave like rigid Euclidean bodies, he could look upon the deviation of bodies from the properties of rigid bodies as the effect of a gravitational field. Then he represented their deformation in curvilinear coördinates.

The components of the Riemann-Christoffel tensor could be used as a measure of the deviation of a given deformation from the motion of a rigid body, just as the curvature of a path is a measure of deviation from straight-line motion.

Hence Einstein formulated his new laws for the gravitational field by setting the components of this tensor to something different from zero and dependent on the surrounding gravitating masses. So arose the Einstein field equations of gravitation.

The logical structure of the march of his thought therefor was quite similar to that which led from Galilei to Newton. Again: first, the creation of mathematical concepts,—then, 'acceleration', now, the Riemann-Christoffel curvature tensor; second, the formulation of a description of a larger realm of facts with the help of these concepts,—then, transition from the constant gravitational field of a free fall to the general gravitational field of Newton, now, the transition from a vanishing gravitational field with a vanishing curvature tensor to any strong field with a curvature tensor depending on the distribution of masses.

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The Lodestone and the Understanding of Matter in Seventeenth Century England

BY

GORDON KEITH CHALMERS

COLUMBUS and Galileo are usually considered the prime revolutionaries whose discoveries in the physical world brought on the spiritual revolution in modern life, but during the first full century of the modern world another discoverer was so regarded by Sir Thomas Browne. In the "experiments, grounds, and causes," of the compass needle, he said, Dr. William Gilbert "discovered more in it than Columbus or Americus ever did by it." Like Columbus, Gilbert made his discovery unwittingly. The navigator had been in search of Asia when he found a new continent; the doctor in seeking a reason for magnetic action led men's attention to reasoning on a new theory of Matter. Through his magnetical researches, the old logical conception of a continuous, soul-animated matter held by the conservative doctors and alchemists was discredited. The discoveries of all three, Columbus, Galileo, and Gilbert, were in many ways interrelated, and of course no single mind released the curiosities of the modern world unaided. What the scientists had to find out in their own laboratories and their own reasoning was persuasive evidence that matter is divorced from spirit, and for a

time in the seventeenth century some of the fundamental hypotheses which indicated the divorce derived from magnetism.

The lodestone had always tried the ingenuity of philosophers. "There is yet remaining the great mystery of the Loadstone," said Sir Kenelm Digby, "which all Authors, both antient and modern, have agreed upon as an undenyable example and evidence of the shortness of mans reach in comprehending, and of the impossibility of his reason in penetrating into, and explicating such secrets, as nature hath a mind to hide from us."[1] Pliny the Younger had asked: In what does Nature show greater waywardness? To some stones she has given a voice, he said, referring to the echo; but to this stone she has given both sense and hands![2] Fracastoro in the sixteenth century wrote a treatise on sympathies and antipathies, listing the famous ancient examples—the cock and the elephant, the wolf and the lamb, cabbage and rue, the bewitching of children, sensation, love and hate, the strange power of the torpedo or electric eel, and the incredible spread of contagion. All these phenomena, he said, are of the same order as magnetical mysteries—the sympathetic vibration of lute strings and the terror of the lion at the crow of the cock as well.[3] Galen's authority was taken for the use of ground lodestone in purgative medicines, and any absorption or an attraction such as the sun for the sun-flower was believed to be fundamentally like the stone's love of iron. Throughout ancient and renaissance science the magnet had given the lie to the cherished dictum of materialistic physics:

Nulla actio a materia fieri potest nisi per contactum.

Thus magnetism involved metaphysics. When Descartes attempted to reduce all cosmology to a logical system in his *Principia* he managed to give a fairly thorough account of earth, the heavens, the seas and their wonders by means of the traditional

[1] A Treatise of Bodies, (1644) London, 1658, p. 218.

[2] *Historia Naturalis*, XXXVI, xxv (xvi).

[3] *De Sympathia et Antipathia Rerum* (1546) *in Opera*, Venice, 1574, p. 58A *recto*. Burton in the *Anatomy of Melancholy* also considers the lodestone in his discussion of the "loves and hates" of inaminate things.

elements, air, earth, fire, and water, but the lodestone required a long section of the final book of his treatise, "for since its power is diffused throughout the whole globe of the earth, there is no doubt that it pertains to this general study."[4] School philosophy avoided every crucial question of physics by naming Qualities. "As, of the sensible qualities," said Digby, "ask what it is to be white or red, what to be sweet or sour, what to be odoriferous or stinking, what to be cold or hot? And you are presently paid with, that it is a sensible quality, which hath the power to make a wall white or red, to make a meat agreeable, or disagreeable to the taste, to make a gratefull or ungratefull smell to the nose, &c. Likewise they make the same questions and resolutions, of Gravity and Levity. . . . The Loadstone and Electrical bodies are produced for miraculous, and not understandable things; and in which it must be acknowledged, that they work by hidden qualities, that mans wit cannot reach unto."[5]

The Aristotelian logic of the physical world had been brought to the service of theology at a thousand points by the schoolmen, and even Sir Francis Bacon was tempted by the lodestone into saving one more physical demonstration of the canonized metaphysics. The question, he said, is whether bodies always "dispose or alter intermediate bodies" in influencing objects at a distance. When the stone draws iron, the intervening air is not changed, neither does the virtue or action reside in the air, he reasoned. "And if the virtue or action has nothing to do with the intermediate body, it follows that there is a natural virtue or action subsisting for a certain time and in a certain space without a body. . . . And therefore magnetic action may be an Instance of Divorce between corporeal nature and natural action." Then Bacon, in his proper rôle of philosophical strategist, maneuvered the argument—"To which may be appended as a corollary or advantage not to be omitted, that here is a proof furnished by merely human philosophy of the existence of essences and substances separated from matter and incorporeal."[6] So in Bacon's

[4] *Principia Philosophiae* (1644) IV, cxxxiii.
[5] A Treatise of Bodies, "The Preface," 1658, pp. B2 *recto*—B2 *verso*.
[6] *Novum Organum*, II, xxxvii (Spedding's translation).

understanding the lodestone demonstrates the scientific possibility of angels, for St. Thomas Aquinas's definition was: "*Angeli sunt formae immateriales.*"[7]

Dr. Gilbert made fun of the mystery mongers, the metaphysicians of the magnet, and all those who "chew the cud of old opinions." Their reasoning, he complained, has been subtler than their observation. In 1600 he published

> *De Magnete, Magnetisque Corporibus, et de magno magnete tellure; Physiologia nova, plurimis argumentis, & experimentis demonstrata.*

The work is blemished by his own ambitious metaphysics proposing a *Physiologia nova* (new physics), but the significant words of the title are the last two: Demonstrated by Experiments! Most of the book is a laboratory report, systematically explaining and relating his own observations of the stone. Although he did not succeed in supplying an acceptable new physics, his accurate account of the hitherto mysterious movements of magnet, iron, amber and straw helped to open the eyes of the next generation to the modern atomic hypothesis.

In ancient times there were two contrary opinions about the magnet, that its action is spiritual, and that its action is material. Galen lived in an animate world. "Everything which exists," he taught, "possesses a faculty by which it attracts its proper quality."[8] Thus grain attracts water, and peasants about to sell their wheat augment its weight by setting jars of water among the sacks. So also cathartic drugs attract their special qualities, and not only these "but also those which remove thorns and the points of arrows such as sometimes become deeply embedded in the flesh." These all show the same faculty as "the famous and far-renowned stone which draws iron."[9]

"We must understand that there are two kinds of attraction," says Galen, "that by which a vacuum becomes refilled and that caused by appropriateness of quality; air is drawn into bellows

[7] *Summa Theologica*, I, 61.

[8] On the Natural Faculties, I, xiv, 55 (English translation by Arthur J. Brock, The Loeb Classical Library).

[9] *Ibid.*, I, xiv, 44–45.

in one way, and iron by the lodestone in another."[10] The magnet draws the iron biologically, as we should say today; air fills the bellows mechanically. So thought Galen. In the *Timaeus*, on the other hand, the magnet is explained by mechanics. There is no attraction, says Plato, but substances shove each other round and round when water is stirred up, when the thunderbolt flies, and in "the marvels that are observed about the attraction of amber and the Heraclean stones."[11]

Epicurus elaborated the mechanical theory, saying that atoms flow from the stone, hooking on to the atoms of the iron and drawing it back. Lucretius, his disciple, gave a more scientific account: the magnet, he said, sends off towards the iron turbulent *semina*, seeds which part the air, leaving a vacuous trail; pressure on the other side of the iron forces some of its atoms along the vacuum until they join the lodestone, bringing along the rest of the atoms of iron.[12] That the lodestone draws iron and not other substances is due to the structure of the iron. Gold is too compact; wood and such bodies are too rare, and the seeds pass right through them, said Lucretius.[13]

After Galen there was very little direct study of the natural world in any science. Lucretius, it is true, saw the magnetic chain—a series of magnetized rings—and rightly concluded that magnetic force is transmitted from the magnet to each in turn. He had also seen the Samothracian rings leap about (*exsultare*) and iron filings, when the Magnesian stone was set beneath the brass bowl where they were contained, move in a frenzy (*fuere*), and so he stated the principle that the stone not only attracts but also repels.[14] These were observations worthy of scientists of the Renaissance. Beside them may be put the experience of St. Augustine, who also saw the *catena magnetica* (he trembled—*vehementer inhorrus*—at the sight!), and who told how the Bishop of Milevita showed him at dinner an iron on a silver plate moved by a stone held beneath.[15] But Augustine went on to report what

[10] *Ibid.*, III, xv, 206.
[11] *Timaeus*, 80 b, c (Jowett's translation).
[12] *De Natura Rerum*, VI, 1002–1041.
[13] *Ibid.*, VI, 1058–1061.
[14] *Ibid.*, VI, 1042–1047.
[15] *De Civitate Dei*, XXI, iv.

he had read about the magnet—"When a diamond is laid near it, it does not lift iron; as soon as the diamond approaches it drops it." Stories and theories accumulated and became more fantastic. Alexander Aphrodisias said that the magnet eats and is nourished by iron; Claudian in his idyll on the magnet said that it lives by iron, and without it thirsts and dies.[16]

To the ingenuity of the Christian philosophers was added in the twelfth century the inventiveness of the Arabs. They explained attraction by similitudes, a version of the theory of sympathies and antipathies, old in Greek science, by which sidereal movements, growth and decay of plants, gravity, cohesion, chemical affinity, and nearly every natural action was explained. The sympathies and antipathies of the magnet, as they were understood in the twelfth and thirteenth centuries, were set down by the Englishman, Alexander Neckam (1157–1217). The attractive virtue (Galen's, essentially), said Neckam, works either occultly or manifestly. Occult virtue is like similitude in its effects, and it acts as scammony when it draws bile and hellebore drawing the vapours. Manifest virtue is perceptible; it is seen when the magnet draws iron and when jet draws chaff. The polarity of the magnet he suggested in the sentence: "The lodestone attracts by one part by similitude and from another part expels by dissimilitude."[17]

The accepted Aristotelian principle that the heavenly bodies are the efficient causes of many motions and shed their influence on this earth was readily attached to magnetism, once a rough compass was invented. "The magnet is found in India," said Cardinal Vitry in 1218, "and it draws the iron to it by a certain occult nature. An iron needle, after it has touched the stone, always turns to the northern star, which does not move around the axis of the heavens as do the other stars; whence it is very necessary to those who navigate the sea."[18] Guido Guinicelli, whom Dante praised, thought that the attractive virtue comes

[16] *Magnes, Carminum minorum opusculorum*, XXIX.

[17] *Quoted by* Park Benjamin *in his* The Intellectual Rise in Electricity, New York, 1895, p. 127.

[18] *Historiae Hierosolimitanae*, LXXXIX, *quoted by* Benjamin, *op. cit.*, p. 154.

from the pole star by way of the magnetical mountains—the nail-drawing mountains of the *Sheharazade*—and from these, said Guido, comes the stone with which the needle is rubbed in order to make it turn towards the Great Bear.[19]

Save a small reference to Thales's opinion,[20] Aristotle had left no dicta on the lodestone; so schoolmen and alchemists were left a free field of fancy, though a mediaeval *De Lapidibus*,[21] probably by an Arab, was circulated under the Stagyrite's name and authority. That treatise does speak of the magnetic poles but perpetuates the old wives' tales about flesh magnets and other wonders. Although they observed nature almost not at all, some of the mediaeval commentators on ancient science did off-set the Galenic and spiritualistic theory of the stone with suggestions of a physical connection between magnet and iron. The Guido whom I have just mentioned said that the magnetic virtue is given to the intervening air; so also Guido delle Colonne. Theirs was a mixture of physical and logical reasoning acceptable to the Peripatetics. Averroes (1126–1198), the greatest of the Moorish annotators of Aristotle, said that the stone has a *certain disposition* when in the vicinity of the iron and that its virtue is transferred to the air and thence to the metal; St. Thomas Aquinas said the same thing.[22]

The early renaissance brought on by Abelard and his contemporaries called forth the modern studies of magnetism. Peter the Pilgrim, Roger Bacon's pupil, examined the stone empirically. He prepared apparatus not only for observation but for experiment, shaping a lodestone into a globe in order to see in little what the great magnetical sphere of our heavens will do to the needle. Unlearned people, said Petrus, suppose that the virtue which

[19] *Cf.* Benjamin, *op. cit.*, p. 155.

[20] *De Anima*, 405a, 19–20.

[21] This work was ascribed to Aristotle by Albertus Magnus and Vincent de Beauvais. Of the "*De Lapide*" Sir Thomas Browne says: "Which book, although we find in the catalogue of Laertius, yet, with Cabeus, we may rather judge it to be the work of some Arabic writer, not many years before the days of Albertus" (Works, ed. Wilkin, 1835–36, II, p. 301). The contemporary edition of "the catalogue of Laertius" contains the item: περὶ λίθων ά (*De Vitis*, ed. Isaac Casaubon, Geneva, 1594, p. 330).

[22] *Cf. Physica*, VII, 3.

attracts iron exists in the mineral veins where the lodestone is found, "whence they say that the iron is moved to the poles of the earth because of the mines of the stone there existing." But such mines are to be found in many places besides the poles, he said, and in spite of their existence elsewhere, the needle is always observed to turn along the meridian circle of the heavens. Thus it turns towards the two points where these circles intersect: that is, towards the north and south poles of the celestial sphere, "wherefore from the poles of the world (i.e., our universe) the poles of the magnet receive their virtue."[23] Peter was a practical as well as a reflective man. He made a perpetual motion machine (though he failed to make perpetual motion) and he planned a magnetic—therefore an electric—motor; his mariner's compass was the first equipped for constant use as compasses are now employed. But his theories were the ancient ones. The magnet, he observed, draws another magnet more powerfully than it draws iron; so here he found support for the old teaching, *similia similibus*.

Still the hypotheses were fantastic, soul or a rational *causa* motivating the needle; and no general physical law was guessed at to describe action without apparent corporeal contact. Meanwhile explorers sailed farther into the dark seas, and a plain account of the needle's behavior, at least, was in serious demand. From Petrus Peregrinus (1269) until Elizabethan times the chief magnetical problems were the variations of the needle and its dip. The primary variation is the needle's deviation east and west from the true north, a deviation which is different in different places, and which was especially difficult to describe as long as mariners reported from strange quarters of the seas the magnetic mountains, rocks, and isles. For the reports were contradictory. Ptolemy's atlas in its successive editions bears evidence of countless disputes along the waterfronts of Europe. Sailors swore they *saw* the mountains in the East, on Elba, in Scandanavia. Each

[23] While entrenched with the forces of Charles of Anjou before Lucera in 1296 Peter wrote his famous letter; almost three centuries later it was edited and printed by Achilles Gasser: *Petri Peregrini Maricurtensis de Magnete, seu rota perpetui motus libellus*, Augsburg, 1558. *Cf.* Benjamin, *op. cit.*, pp. 177, ff.

new atlas recorded the latest certainty; finally the map showed them standing in the frozen seas of the Arctic not many leagues from what we now call the magnetic pole.[24] The needle has also a secular variation in a given spot from year to year. This puzzled Columbus sailing west of the Azores and so terrified his crews when the secret of the bewitched needle got to the forecastle that to quiet them he had to invent a tale that the pole star had moved from its place.

The dip is the declination of the north-seeking point as the balanced needle travels northward on the earth's surface and its converse declination when moved southward, a phenomenon easily shown with a needle and a spherical magnet. At the poles the needle stands erect. Studies of the dip led Robert Norman, an Elizabethan maker of nautical instruments, to say that the "Respective Point" towards which the needle turns is in the earth, not the heavens.[25] On the manner of attraction Norman had nothing to say. The needle is stroked with the lodestone; "this stone," he said, "hath wholly and fully himselfe the Power, Action, Propertie and Vertue of his own Appetite to shewe and to cause the Needle to shewe the point Respective, without any Attractive qualitie or external cause of Rockes of the Magnes stone, or by Attraction in the Heavens or elsewhere whatsoever." But if you ask him what the Appetite is, he answers, "I am no more able to satisfie you herein, than if you should aske me howe and by what means the celestiall Spheres are moved." The directive virtue is in the needle, derived immediately from the stone, where "God in his Omnipotent providence hath appointed it so to bee." When he reasoned on the transferrence of the magnetic virtue from the stone to iron, from iron to a nail and thence to

[24] Pliny and "Serapion the Moor" (Yuhanna Ibn Serapion Ben Ibrahim—Serapio Mauritanus) are the authorities for the older belief that the mountains are in India. It is dangerous, said Pliny, to sail near those coasts in ships fastened with iron nails! Saxo Grammaticus and, on his authority, Olaus Magnus, Archbishop of Upsala, set them near the north pole. In order to pass them, he says, a ship must be constructed with wooden pegs (*Historia de Gentibus Septentrionalibus*, Rome, 1555).

[25] The Newe Attractive, containing a short discourse of the Magnes or Lodestone, and amongest other his vertues, of a newe discovered secret, and subtill propertie concernyng the Declinyng of the Needle, touched therewith, under the Plaine of the Horizon . . . , London, 1581. *Cf.* Benjamin, *op. cit.*, pp. 217–219.

another nail (the *catena magnetica*) Norman was more scientific. It is like musk, he said, which, "having a sweet savour or smell itself imparteth the same to another thing, as to a pair of Gloves: and those Gloves give out savour and perfume a whole Chest of Cloaths." A century later Robert Boyle could imagine that the odour of musk is nothing but particles of musk sent forth; by that time it was possible to think of a magnetic off-flow similarly composed of particles.[26]

The "father philosopher" of magnetism, William Gilbert, called Peregrinus's sphere the *terrella*, little earth, and applied to it all the observations of the mariners who reported the strange behavior of the needle. Our earth, he concluded, is a great magnet;[27] and on this discovery he constructed a whole Copernican cosmos held together by magnetism. Though his conclusion led him into wild conjectures about the souls of the worlds, Gilbert's premise suggested experiments on the de-magnetization and re-magnetization of irons and many useful hypotheses of the needle's behavior. Just as iron receives "verticity" by being laid toward the poles of the *terrella*, he found, so it receives "verticity" from the earth when set in a north and south position. The lodestone is best preserved, he noticed, if surrounded by iron filings. The kinship of iron with lodestone led him to study ore in various states and to observe that ore acquires poles and arranges itself with reference to the earth's axis. If you heat ores and let them cool they acquire verticity; wrought iron not magnetized nevertheless attracts. In truth, the lodestone and iron are very similar; they have "affinities as though they were blood relations."[28] On the descriptive side the magnetical science was making progress.

Since Plato's time the electric attraction of rubbed amber for straw and the magnetic attraction of the Heraclean stone for iron had been confused. Jerome Cardan in the sixteenth century saw the difference, and Gilbert was able to deal with the behavior of

[26] *Cf.* Boyle's Essays of the Strange Subtilty, Great Efficacy, and Determinate Nature of Effluviums, London, 1673.

[27] *De Magnete*, I.

[28] *Ibid.*, I, xiv.

electrics and magnets as separate sciences. Cardan was heir to all the sidereal and sympathetic mysteries, yet he proposed a theory of electric attraction, which was to play an important part in the great seventeenth century idea of atomism. The attraction of the amber, he said, is caused by "a fatty and glutinous humor."[29] This is moist, said Cardan, and the straw is dry; dry desires moist, so moves towards it—sympathies again. Yet the humor was a definite physical thing; in Gilbert's reasoning it was to become the effluvium of electrics, in Sir Thomas Browne's it was to suggest a stream of atoms.

Cardan erred, said Gilbert, in assigning sympathies as the cause of electric attraction: electrics attract every kind of substance, and anyway, like does not attract like—does flesh attract flesh? Gilbert examined amber, jet, diamonds, rock crystal, mica, glass, and other electrics, coming to the conclusion that the whole species of electrics are substances concreted out of humors—firmly concreted. Rubbing releases the humors in the shape of natural emanations or effluvia, and these cause attraction. Experiment shows this effluvium to be gross, for a mist on the amber will check it, and other matter such as paper, linen, sarsenet, and brandy intervening will prevent electric attraction.[30] On the other hand, the effluvium is subtle enough to penetrate olive oil, when that is spilled on the surface of the amber. And the electric effluvia are more tenuous than the denser, fire-containing effluvia of flint; for the latter, on being struck off, make sparks, but those of rubbed amber are so minute that they are not fit matter for flame.[31] Also, they are fine enough to pass through the air without disturbing it, as one can see who puts rubbed amber near a lighted candle; furthermore, they will draw a water-drop up into a cone. If they moved the straw by rarefying the air in front of it, the straw would move irregularly and sidle, but in fact it moves all at once, showing that the effluvia go forth directly from amber to straw. They spread everywhere, Gilbert concluded; deriving from the original humor out of which the electric was concreted, they are set loose as material rods.

[29] *De Subtilitate*, Paris, 1551. Quoted by Benjamin, *op. cit.*, p. 248.
[30] *Cf. De Magnete*, 1600, pp. 53, 56.
[31] *Ibid.*, p. 55.

Magnetic action, by contrast to electric, can be interrupted by no obstacle in the way, not even slabs of marble. Water, alcohol, nothing—not even garlic, from ancient times believed to destroy the action of the lodestone utterly—will stop the emanation from magnets. Therefore, Gilbert decided, the magnetic off-flow is immaterial, an "incorporeal going forth of the Form." Living at the end of the sixteenth century Gilbert could not abstain from using the mystical contrivances always ready at hand to do the most ticklish jobs of physics. The wierd metaphysical Form which Gilbert invented to account for the apparently generative properties of the magnet would be interesting to observe if space allowed. The lodestone inspires magnetism in dead iron, he reasoned; so it is a kind of creator (Gilbert said an arranger, ordering the chaos of the iron or giving it form). The Form of the magnetic *vis* behaved like the mediaeval Active Intellect,[32] giving rise in Gilbert's mind to his system of souls—souls of the magnets, soul of this earth, and souls of all the heavenly bodies, which, he believed, are held in place and given their motions by magnetism.

The "magnetical philosophy" of the earth was so fantastic that to a degree it vitiated the scientific usefulness of Gilbert's *De Magnete*, and Sir Francis Bacon, a bigotted anti-Copernican, laughed at Gilbert, saying, "He has ascribed too many things to that force and built a ship out of a shell." Gilbert's theory of the manner of magnetic action offered little to the chemists, doctors, and philosophers groping for a means of describing physical motion in material terms, but his laboratory observations were so exact and so well systematized that his experiments served them in spite of his conclusions. At one point he did hint at a material explanation of magnetism. Throughout his book he reiterated that the going forth of the peculiar magnetic Forms which "keep and order their own globes" is incorporeal, contrasting this motion in almost every chapter with the material emission of electrics. That emission Gilbert had borrowed from chemistry, where a humor was an observable vapour, odour, or breath

[32] *Cf.* Maimonides, Guide of the Perplexed, II, iv; also St. Thomas Aquinas, *Summa Contra Gentiles*, II, lxxxix.

(*spiritus*) released by heating substances in the alembic. It might be thought, said Gilbert, that there is a spiritous but corporeal ingression into the iron which changes it like the vapour or odour of lead and of gold in the alembic—can there be a *magneticus odor*?[33] Inability of all resistant bodies interposed to hamper the action disallows it, said he. Nor are the magnetic forces due, as Baptista Porta thinks, to infinitesimal parts of the stone which go forth as little hairs. They go forth, in truth, said Gilbert, in a way analogous to light: they arrive instantaneously; they do not remain in the atmosphere above the vapours or effluvia of the earth, nor are they caught by air or water; like light, they do not rush upon anything, but solicit friendly bodies; in the absence of its source the magnetic force disappears. But the magnetic vigour surpasses light in being subtler and never interrupted by opaque bodies.[34] So he concludes that the magnetical off-flow is not even substantial in the mediaeval sense of substance. But he allows one connection with matter. In describing the going forth of the magnetic forces from the souls of heavenly bodies he says that the immaterial effused Forms (*immateriales effusas formas*) are borne and carried by a breath (*spiritu feruntur, & efferuntur*).[35]

In the direct line of development of atomic theory, Gilbert does not appear to hold an important place. He did nothing to analyze or describe the atom itself. Yet the twenty or more solutions to magnetical enigmas presented in his book provided the real occasion for much of the subsequent experimental work on the theory of Matter. Sometimes in the history of thought, theory has begotten theory, but more often fact has been the parent of the best hypotheses. The deposit of theory inherited by the Renaissance was soured and fermented; what science needed was freshening fact. Gilbert's demonstrations of the needle and terella could be reasoned upon, and they supported Descartes, Boyle, and the followers of Gassendi in the most fundamental hypothesis of their physics, the theory of corpuscles or atoms.

[33] *De Magnete*, p. 66.
[34] *Ibid.*, p. 77.
[35] *Ibid.*, p. 210.

In the first part of *Pseudodoxia Epidemica* (1646) Sir Thomas Browne devoted three long chapters to Gilbert's experiments on the lodestone and amber, elaborating Gilbert's laboratory practice with further observations and new apparatus. Before Browne was done with magnets he had followed his own behest—

> Let thy Reason go
> To ransome Truth, even to th' Abyss below.

He read all the magnetical literature, ancient and modern, criticised the reports and theories of seamen, and analyzed all the great hypotheses of electrical[36] and magnetic action. His magnetical researches gave Browne an inkling of Atomism and the material physics which characterizes our thought since the Renaissance, suggesting to his mind whole volumes, still unopened, in the Book of Nature.

The speculative philosophers of Browne's time had offered three theories of magnetic action; Gilbert had proposed another. The most orthodox was Cabeo's. Nicola Cabeo, a Jesuit (*Philosophia Magnetica*, 1629) set out to overcome the heresies of Gilbert; he refused to believe that iron can be trans-Formed into Lodestone, for Avicenna, in a tract based on the pseudo-Aristotelian *De Lapidibus*, had pronounced that it is not in the power of the alchemists "to bring about any true change of species."[37] The earth, Cabeus had to admit, has magnetical *vis*, but it is no perfect magnet. Accepting as he was forced to do Gilbert's demonstrations of the behavior of the needle and *terrella*, he brilliantly adapted the Aristotelian physics to account for the cause and manner of magnetic action. When he was done with the lodestone, its *vis* remained its *essentia* or *forma magnetica*, something far different from Gilbert's animating Form—indeed, a mere *qualitas*. This is a most general thing, said Cabeus, like the quality of gravity or of any other motion, a *qualitas motiva & alterativa*. It brings about magnetic coition (*concursus*) by the

[36] Sir Thomas Browne is credited with the first recorded use of the word *electricity* in English.

[37] *De Congelatione et Conglutinatione Lapidum*, translated from the Arabic by E. J. Holmyard and P. C. Mandeville, Paris, 1927, p. 41.

reputable Aristotelian method of perfecting itself.[38] The effluvia of the magnet are neither bodies nor Forms, he said, but *qualities*. That theory of the magnet is Peripatetic. Bacon's was spiritual, being derived from Galen—by its *natural faculty*, said Bacon, the lodestone moves towards iron and iron towards lodestone.[39] Cabeo's theory was logical, Bacon's biological and spiritual, Gilbert's half spiritual, half physical. The fourth theory was the materialistic one of Descartes and Sir Kenelm Digby, which contributed very considerably to seventeenth century Atomism.

There is not space here to describe the antecedents of the materialistic theory of magnetic action nor to dwell upon the significant differences between the materialism of Descartes and that of Sir Kenelm Digby, but in reading the summary of their hypotheses which follows one should bear in mind that these two speculative thinkers had little applied science and did nothing more for the magnetical philosophy than reason upon the observations of other men. They made no small contribution to general physics, however, by applying to magnetical knowledge both the ancient theories and the hypotheses derived from other sciences—from the medical and chemical sciences especially. Doctors and chemists had been relying more and more upon small particles and atoms to describe all mysterious and invisible reactions. They were concerned chiefly with specific diseases and chemical combinations, but some descriptive terms and a few rudimentary theories they borrowed direct from the general physics of Epicurus and Lucretius. In practice they had come to regard the ancient *spiritus* of the Stoical physics as a corporeal, and eventually as a particulate emission. Fracastoro's discovery of seeds of the plague in 1546[40] suggested a sort of particulate action in physiology; Daniel Sennert in 1635 described the simples of chemistry as "*minimae naturae, atomi, atoma corpuscula, σώματα ἀδιαίρετα, corpora indivisibilia*," so minute that the senses cannot be aware

[38] *Philosophia Magnetica*, 1629, pp. 41, 312–313.

[39] The *Anima Mundi* of the Stoics was the most popular agent of physical and physiological change among doctors, alchemists, and all other practicing philosophers of the time.

[40] *De Contagione et Contagiosis Morbis et eorum Curatione, Libri* III, Verona, 1546.

of them.[41] Both a theoretical and an experimental atomism was in the air—not exactly the ancient Greek belief that matter is composed of indivisibles, but a rough compromise with that idea, namely, that minute bodies make up matter. The Renaissance had constructed its own science of *effluvia*.[42] In 1649 Pierre Gassendi was to rationalize the new experiments of all sciences under the complete atomic system of Epicurus.[43]

Descartes and Digby applied the corpuscular hypothesis of matter to magnetism. Kepler was the chief teacher. Adopting Gilbert's magnetical hypothesis of the earth Kepler taught that the sun has a great sphere of magnetic virtue revolving about it. The planets, he said, are carried in this vast whirlpool, and they are prevented from being drawn inward to destruction only because the sphere of virtue is made up of fibres or filaments. The fibres are straight, and they drag the planets about in magnetic streams like ships in a maelstrom.[44] Kepler's magnetic maelstroms are the ancestors of the Vortices which supplied Descartes with a mechanical explanation of the cosmos—the first purely physical account of the world in modern times. Suggested at the outset by a magnetical theory the Cartesian automaton of the cosmos assigned an important place to the lodestone. In Descartes's mind, however, the original motion of this world was not magnetic but a fillip by the hand of the Almighty.

The original solid, uniform Matter, said Descartes, was divided into polygonal masses. These, in circular motion, ground off the corners and produced spheres. The filings and scrapings made the transparent substance called aether, out of which the space between the planets is made, and the whirling also produced luminous bodies such as the sun and stars and the opaque bodies,

[41] *Hypomneta Physica* (1635) *in Opera Omnia*, Venice, 1641, Vol. I., p. 17b.

[42] For a discussion of the history and meaning of the words, *effluvium*, *effluxion*, and *emission*, see my article, Three Terms of the Corpuscularian Philosophy, Modern Philology, XXXIII, 3, February, 1936.

[43] *Animadversiones in decimum librum Diogenis Laërtii, . . . (Appendix . . . Appendix altera, quae est Philosophiae Epicuri syntagma.)*, Lyons, 1649. For reference to Gilbert's observations, *cf. Syntagma*, III, I, iii, 5. The *Syntagma* is Gassendi's great work; it occupies two folios of the six-volume *Opera* (Lyons, 1658).

[44] *Cf.* Kepler, *De motibus stellis Martis*, Prague, 1609 and Benjamin, *op. cit.*, p. 354.

earth and moon.[45] The parts of the universe revolve in currents or vortices, Descartes thought, with a luminous body at the centre of the aether which surrounds it. Minute particles of the parings must escape through the narrow openings between the revolving spheres, he said, and consequently they take the shape of spirals.[46] They penetrate appropriate bodies by pores ready to receive them, and this is the motion which causes magnetism. Because of their shape, the spirals can enter the passages of the lodestone in only one direction; they go parallel to the axis, entering at one end and leaving at the other, whence they swing around through the air and enter again at the proper opening. So the magnetic force travels through the stone from South to North, through the air back to the South pole, and again through the magnet. The spirals which will fit the northern openings, said Descartes, go in the opposite direction—[47] that is, there are left- and right-hand screws.

This application of the vortical motion to the magnet was probably suggested by Cabeo's studies of the lodestone's poles. Using long shreds of iron, Cabeus demonstrated that the magnetic vigour centres in the magnet unevenly, gathering in radiating lines from either pole. Descartes conceived of the force going through the stone and around in the air to repeat the journey over and over again. His streams of spirals accounted for all magnetical phenomena, even the magnetism of the earth. The attractive power of the earth is weak for so large a magnet, said Descartes, because near the surface the spirals encounter a stratum of metals with appropriate pores, and into these many of them stream.

Sir Kenelm Digby during one of his exiles in Paris met Descartes's friends, probably the philosopher himself, and stole some of his ideas, publishing in 1644 (the same year as Descartes's *Principia*) *A Treatise of Bodies*, which presented a kind of Epi-

[45] *Principia Philosophiae*, III, lii.

[46] In a letter to Henry More Descartes called the particles *particulae striatae*; Descartes's friend and editor translated the term, *parties cannelées* (Descartes to More, Egmont, April 15, 1649).

[47] *Principia Philosophiae*, IV, cxlvii.

curo-Cartesian description of matter. Pierre Gassendi was preparing his atomistic *Syntagma* (published in 1649) while Digby was abroad, and elements of Gassendi's system appear in Digby's treatise side by side with principles from Gassendi's enemy, Descartes. Digby called Descartes's spirals (*particulae striatae*) by the ancient name, *atoms*. He had the ingenious idea that atoms rise in the torrid zone attracted by the heat of the sun. The cold particles moving from the poles towards the equator meet the hot dry atoms, he thought; thereupon they conglomerate and fall to the earth as stones; and in these the atoms continue their north-and-south motion. The stones, of course, are magnets.[48]

Sir Thomas Browne had studied his medicine at Montpelier, where Galen was still the authority and his principle of natural faculties still did service in all physics. Sir Thomas held Galen in high veneration throughout his life, and it would have been most natural for him to apply to magnetism the Galenic theories of action without corporeal contact—the theories which satisfied Sir Francis Bacon. Indeed, he could have carried out all the experiments of Gilbert in the spirit of an antiquarian virtuoso and still have maintained the old loyalty to so honourable a physiology as Galen's. That was the accepted way in the seventeenth century for a doctor and scholar to behave. But the scholar in Browne could not down the scientist. He watched his lodestones, his straw and amber long hours, speculating, speculating, to find an hypothesis which would most reasonably "salve" appearances. The invisible connection between stone and armature, he decided, is material.

> Now whether these effluviums do fly by striated atoms and winding particles, as Renatus des Cartes conceiveth, or glide by streams attracted from either pole and hemisphere of the earth unto the equator, as Sir Kenelm Digby excellently declareth, it takes not away this virtue of the earth; but more distinctly sets down the gests and progress thereof, and are conceits of eminent use to salve magnetical phenomena.[49]

[48] A Treatise of Bodies (1644), London, 1658, pp. 218–224.
[49] *Pseudodoxia Epidemica*, 1646, II, 2.

So said Sir Thomas Browne at the end of his review of Gilbert's hypothesis that the earth is a master magnet. The assertion was more than a report of one among many possible theories, for the principle that streams of minute particles pour forth became for Browne a general principle of magnetism.

Browne's own experiments on irons and the needle are interesting, but what concern us here are his conclusions about how the magnet works. These he not only summarized but elaborated in comments on his laboratory activities. Gilbert had thought of light in scholastic terms as something immaterial, but in Browne's science the Visible Species stream forth "in corporeal rays."[50] The magnetical effluvia, said Browne, are like them—

> For these effluxions penetrate all bodies, and like the species of visible objects are ever ready in the medium, and lay hold on all bodies proportionate or capable of their action.[51]

Sometimes he thought of the effluvia as spirals. Winded effluxions, he said, provide an "untouched" needle with "verticity" when it is hung above a lodestone—

> Now this direction proceeds, not primitively from themselves, but is derivative and contracted from magnetical effluxions of the earth, which they have winded in their hammering and formation, or else, by long continuance in one position.[52]

At other times he called the particles *atoms*. The influx after heating supplies irons with "direction," he thought:

> For by fire they amit not only many drossy and scorious parts, but whatsoever they had received either from the earth or lodestone; and so being naked and despoiled of all verticity, the magnetical atoms invade their bodies with more effect and agility.[53]

With Descartes, Sir Thomas conceived of particles streaming out of the earth's poles and along the axis of the lodestone.[54] If there

[50] Works, *ed.* Wilkin, Vol. II, p. 418.
[51] *Ibid.*, Vol. II, p. 286.
[52] *Ibid.*, Vol. II, pp. 287–288.
[53] *Ibid.*, Vol. II, p. 288.
[54] *Ibid.*, vol. II, p. 290.

were any magnetical mountains, said Browne in effect, they would draw the lily on the tip of the needle by means of magnetical atoms![55]

The historical importance of Browne's conjectures on the manner of magnetic action is great indeed, for Sir Thomas was a laboratory scientist, so recognized by his contemporaries.[56] The general hypothesis of particulate matter was in all its essentials ancient, but the Renaissance would have none of it until practical chemists and physicians showed its use in "salving" phenomena. So far as I know Browne was the first scientist to bring together Gilbert's excellent laboratory practice and the fruitful reasoning of the mechanists and the atomists—at least he was the first to publish a generally accepted scientific account of the new eclectic theory. Also—and here is the all-important contribution of Sir Thomas to modern science—for him the hypothesis of particulate effluvia transcended magnetical problems:

> And truly the doctrine of effluxions, their penetrating natures, their invisible paths, and insuspected effects, are very considerable; for, besides this magnetical one of the earth, several effusions there may be from divers other bodies, which invisibly act their parts at any time, and, perhaps, through any medium; a part of philosophy but yet in discovery, and will, I fear, prove the last leaf to be turned over in the book of nature.[57]

The general principle of particulate effluvia led to the liberation of natural science. Matter and Spirit were separated. By this principle Physics broke with the Stoical hylozoism (matter imbued with a "plastic principle") and also with the scholastic abstraction, *materia*.

Modern Matter began with the "Corpuscular Philosophy"[58] of Robert Boyle. When in 1673 he wrote his *Essays of Effluviums*, Boyle, like Browne, found the "doctrine of effluxions" "very

[55] *Ibid.*, Vol. II, p. 312.

[56] On Sir Thomas Browne's standing as a scientist *cf.* my article, Sir Thomas Browne, true Scientist, Osiris, II, 3, April, 1936.

[57] *Ibid.*, Vol. II, pp. 286–287.

[58] Some Specimens of an Attempt to make Chemical Experiments Usefull to Illustrate the Notions of the Corpuscular Philosophy *in* Certain Physiological Essays, London, 1661.

considerable." He approached the doctrine by means of chemical combination, the odour of onions and musk, the theories of sight and hearing, the sparks struck off from flint by steel, and especially by the new theory of magnetism explaining physically the historic mystery of action without corporeal contact. Modern Matter and the modern physical world were born. But the "leaf . . . in the book of nature" is still unturned.[59] Physicists today remark that on Mondays, Wednesdays, and Fridays they hold the wave theory of light, and on Tuesdays, Thursdays, and Saturdays, the corpuscular theory. The whole duty of some of the best of them in our century appears to be to discover the unsuspected effects of effluxions.

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[59] Besides the works just noted *cf.* Boyle, An Essay of the Great Effects of Even Languid and Unheeded Motion, London, 1685.

Schools of Psychology: A Complementary Pattern[1]

BY

SAUL ROSENZWEIG

"If, then, we are really to find out what is true and what is false, we must direct our attention to the rôle of any particular item in the whole of which it is a part."
—WERTHEIMER: *On Truth.*

I

IN THESE days of *Gestalttheorie* it seems not untimely to inquire whether psychology itself does not stand in need of being recognized as a configuration of complementary parts. The suggestion, it is true, tends at the first to be repudiated by the implacability of the schools with their claims and counterclaims resounding on every side. On further analysis, however, such controversy seems to acquire the very different significance of having permitted certain partial aspects of the whole field to become well-articulated, even if in somewhat exaggerated form. In other words, if there is any underlying unity in psychology, then the very overstatements of the school zealots while unwittingly defeating their own purpose may have been doing the science a real service by throwing its basic pattern into bold relief. In broadest terms this seems to

[1] Presented before the Psychology Seminar, Worcester State Hospital, November 15, 1934 and before the Psychology Club, Skidmore College, February 25, 1936.

describe what has been happening in psychology in the recent past and to define the present stage of its development.

II

An examination of the contemporary schools of psychology reveals certain relationships among them which are schematically shown in the accompanying chart. To be understood properly this chart should be regarded merely as a condensed and approximate representation of the situation portrayed. Criticism of its details would then be beside the point—the point being simply that the schools complement each other more than they conflict.

THE COMPLEMENTARY PATTERN OF PSYCHOLOGICAL SCHOOLS

Name of School	Structuralism	Gestalt Psychology	Functionalism		*Geisteswissenschaftliche* or *verstehende Psychologie*
			Behaviorism	Psychoanalysis	
Characteristic problem or field of research	Sensation	Perception	Learning	Motivation	Character
Temporal span of observation in typical methods	·	○	→	⟶	⟶∞
Conceptually allied sciences	PHYSICAL E. g. Physics		BIOLOGICAL E.g. Physiology		SOCIAL E.g. Sociology

The schools included in the discussion have been selected on the basis of their having been espoused as isms militantly opposed to other contemporary standpoints. These schools seem, moreover, to be typical, i.e., the remaining points of view in the psychological forum of the present day, some of which may be referred to in passing, are readily derived as sub-varieties of these leading genera.

In the following account no attempt will be made to give an exhaustive exposition of the schools or even a complete demonstration of the truth of the assertions made about them. The

reader will be able to fill in by himself the details of this necessarily brief presentation.

1. Structuralism is regarded as a current school because, while its most rigorous protagonist, Titchener, is no longer alive, his position is still represented in several university chairs and is regarded in many quarters as the typical brand of "academic psychology." Historically this school is a direct descendant of the experimental psychology founded by Wilhelm Wundt and foreshadowed in Fechner's work on the psychophysical problem.

Gestalt psychology (the Berlin school) is championed by Wertheimer, its founder, and Köhler, Koffka and Lewin. Wertheimer's original studies of the phi-phenomenon gave rise to a theoretical departure from structuralism. Since then the opposition of this school to behaviorism has become equally emphatic.

Functionalism—a term originally used to designate the psychology of William James and the Chicago school associated with the names of Angell and Dewey—has much in common with the American philosophy of pragmatism. Mental testing, experimentation on animal behavior and the study of personality maladjustments, all naturally derive from this basically biological standpoint. Mental testing never became an ism because its interest was a practical rather than a theoretical one, but behaviorism, formally founded by Watson and since fostered with modifications by Hunter, Hull, Tolman and Skinner, among others, is a definite school with its chief camp in the animal laboratory.

Psychoanalysis, the creation of Sigmund Freud, began as an attempt to solve the problems of abnormal psychology, particularly hysteria. While not in any historically direct way related to American functionalism, it is not difficult to trace both the psychology of James and that of Freud back to certain medical roots that first found expression in French psychopathology. Of near kin to this tendency are the views of Adolf Meyer and William McDougall, both medically trained.

Geisteswissenschaftliche or *verstehende Psychologie* is exemplified in the writings of Spranger and Jaspers and derived from the philosophical positions of Dilthey and Simmel. It arose as a study of culture—regarded as the historical expression of certain

ideal values or types of humanity—and has continued its development in much the same direction.

2. If with even this very sketchy historical synopsis in mind the second row of the chart is next considered, it appears that the entries have been somewhat anticipated. This is so because in attempting to ascertain what could rightly be called the characteristic *problem or field of research* of the schools their historical origins were consulted as one criterion. The other criterion was the published record of work accomplished. None of the schools has confined itself to the problem assigned in the chart—had it done so it could never have regarded itself as an adequate school of general psychology—but by either denying that certain problems were problems of psychology (e.g., behaviorism's attitude towards conscious sensation) or by interpreting them in special ways, occasionally to the point of distortion, they have managed to encompass the whole field of psychology while actually doing research in only a limited part of it.

If these qualifications as well as the two criteria previously mentioned are borne in mind, the statement that the special problem of structuralism is sensation will probably not be disputed. To appreciate its truth one has only to recall the important place of the Weber-Fechner function in this school and the extensive work of the Wundtians and the Titchenerians on the various sensory modalities.

The peculiar problem of Gestalt psychology is perception, as is shown by the early studies of the phi-phenomenon and the numerous subsequent publications in *Psychologische Forschung* as well as by the inescapable patterns of dots and lines in the writings of this school. If one starts to question this statement in the light of Köhler's study of learning in apes, for example, a little thought about the problem in the present setting soon discloses that 'insight learning' is much more a matter of the organization of the *perceptual* field than it is of learning and that it is only for this reason that Gestalt psychology seems to have so much to do with learning.

The same is true of Lewin's work on motivational topics. By omitting genetic considerations and treating the problems purely

on the phenomenological level—in terms of 'the momentary situation'—he is actually talking the language of perception and apperception. His topology seems almost intended for the making of such a translation. This interpretation, it is true, overlooks certain aspects of Lewin's work of which this view cannot be taken, but it seems a sufficiently accurate characterization to establish the point that the typical problem of Gestalt psychology is perception.

In passing on to functionalism a quite different general problem proves to be of central importance—the problem of biological adaptation. In opposition to the existential standpoint of the structuralists these psychologists are interested in the survival significance of various forms of activity. For behaviorism this turns mainly into a study of learning, as the ever-present rat-in-the-maze and the inevitable conditioned reflex tend to demonstrate. The *means* whereby biologically useful forms of behavior are acquired is its chief concern. Mental testing, which has grown up in close association with behaviorism, has certain features in common with the latter since both are occupied with the measurement of efficiency expressed in terms of behavior.

Psychoanalysis, while oriented in very much the same direction, shifts the emphasis slightly more towards the *meaning* of behavior—still biologically considered—so that one finds motivation to be its characteristic problem. The close relationship to behaviorism is easily appreciated, despite this difference in emphasis, if one notes that from the Freudian point of view neurosis is largely a result of faulty learning or bad training; of unhealthy 'forgetting' (repression); of fixated habits. Symbols are, moreover, easily construed in the terms of conditioning. In fact, it is not at all difficult to conceive of an intimate liaison being effected between behaviorism and psychoanalysis,[2] as would probably happen of its own accord except for the inevitable difference between an experimental and a clinical discipline.

Finally, the typical problem of *geisteswissenschaftliche* or *verstehende Psychologie* is character. Culture, regarded as a supra-

[2] As a matter of fact several such attempts, e.g., that of French (5), have already been made.

individual phenomenon, is studied by the methods of *Verstehen* or *Erkennen*. These are opposed to the elementaristic and analytic procedures of natural science and by their use it has been found possible to isolate certain ideal types of human value or personality. It is maintained that simpler psychological events can acquire significance only in the context of such general types or the principles underlying their construction. Spranger has elaborated these concepts in relation to the normal personality while Jaspers has applied them to psychopathology. Both the concern with types and with abnormal mental phenomena suggest the kinship of this school to psychoanalysis.

It would thus seem that each of the schools of psychology is chiefly concerned with a different problem or field of research and that the difference of opinion among them may be largely attributed to this difference in interest.[3]

3. The next row of the chart calls attention to the fact that the temporal span of observation in the typical *methods* employed by the schools in attempting to solve their problems varies in a systematic manner. Whether the method determines the problem or the problem the method or whether both are determined by some third variable need not be decided for the present purpose.

Structuralism, as is well known, has always insisted upon a passive type of introspective observation in which the momentary impression is of chief importance. When the temporal process plays a part in this method it does so only to the point of admitting the immediate present of experience. The chart represents this attitude by a dot.

Gestalt psychology broadens the temporal scope of observation by insisting upon non-local properties of the observational field and by positing certain equilibria or temporal organizations which are fundamental to the events studied. Still the phenomena to which attention is paid belong to the present. This is, for instance, one of the main differences between Lewin's work on motivation and that of the psychoanalytic school in which the

[3] Heidbreder (6, pp. 421–422) has come to the same general conclusion.

past history of the organism is subjected to the closest scrutiny. The Gestalt point of view is represented in the chart by a small circle.

With functionalism the temporal process takes on new significance. In the study of sensation and perception, development, genetic background, etc., do not usually play a great part. For the problems of learning and motivation, however, these factors are highly important. When a behaviorist, for instance, studies the effect of successive repetitions in the running of a maze by a rat, times—and time—cannot be neglected. The temporal span of observation is still fairly limited, nonetheless, in comparison with that of the analyst who in his studies of motivation delves into the past history of the individual back to the earliest years of life. The arrows of different lengths in the chart signify the time-span of these methodological approaches.

Even broader sweeps of time—whole ages of history—are encompassed in the investigations of the *verstehende* psychologists. This extension is in fact so great that, as in the delineation of ideal types, the temporal relevance of the constructs dwindles to practically nothing: they are, so to speak, timeless. This situation is indicated in the chart by the long arrow and the infinity sign.

These observations, however incomplete and however approximate they may be, suggest that the relative complexity of the phenomena treated by the different schools represents a graded series. It should be clear that space as well as time might have been referred to in this discussion with practically the same result, since the essential point lies in recognizing the increasing complexity of the problems studied by the several schools.

4. This same fact is reaffirmed by a consideration of the sciences conceptually allied with the various schools—the sciences to which they turn for inspiration, corroboration and *explanation*. These sciences range from the physical through the biological to the social, and the schools, as the chart indicates, range along in parallel.

Structuralism and Gestalt psychology are most closely allied to physics and chemistry. Consider, for example, the psycho*physics* of structuralism and the attempt of Köhler in *Die*

physischen Gestalten (8) to apply common principles to psychology and physics.

Again, behaviorism has its most intimate relationship with physiology, as Watson's continued efforts to give physiological explanations for all behavior indicate. Kempf's attempts to integrate psychoanalysis and physiology exemplify a similar bent in the Freudian school. In general, however, psychoanalysis leans very little towards this type of explanation, consorting more, even if not for the definite purpose of explanation, with anthropology and the social sciences.

Finally, *geisteswissenschaftliche Psychologie*, as its name proves, is most nearly allied with the cultural sciences, including sociology, history and certain branches of philosophy.

The gamut of the sciences is thus completely run through and the schools of psychology are found to move with it from one end of the series to the other in a rather systematic way. One is tempted to conclude that the province of psychology, construed in broadest terms, is unique in being co-extensive with all the other scientific disciplines. From this point of view it could be maintained that there is a psychological aspect to events of every degree of complexity and that psychology should define its province and its subject-matter accordingly.

But whether this position is or is not adopted, it is at least apparent that the several contemporary schools of psychology can be very plausibly arranged, as in the present chart, to represent a continuum or hierarchy from the less to the more complex, each school having a characteristic problem, a special methodological position in respect to the temporal span of observation and a preferential alliance with the other sciences. A relatively simple underlying pattern would thus appear to unite the warring factions in psychology and the disagreement among them would have to be regarded as largely arbitrary and unnecessary.

III

While it is not at present possible to discuss in detail the further implications of the situation just depicted, some of the questions

that most readily come to mind and tend to linger may be mentioned.

1. With regard to *the nature of the situation itself*, in what sense are the schools of psychology complementary? Do they complement one another in a more or less external manner, as do the other sciences severally taken, in describing a rather heterogeneous universe? If so it would be necessary to conclude that no single set of concepts is adequate for all the phenomena now encompassed by the schools even though the individual sets employed in the different realms of study do not contradict each other.

As a second possibility: does the complementary pattern of the schools imply an intrinsic unity which with sufficient discernment could already be reconstructed or which must remain for the future to disclose?[4]

2. With regard to *the cause for the existence of schools*—each of which regards itself as exclusively valid when they all actually complement each other—can it fairly be said that the exponents of the schools have been committing a 'fallacy of arrogation,'[5] i.e., exploiting their concepts by unduly subordinating to them phenomena for which they were not originally intended and for which they are not really adequate?[6] In a young science like psychology such an error might very naturally be made. Youth tends to be over-enthusiastic, partly perhaps as a compensation for the usual insecurity of adolescence.[7]

[4] In this connection it is worth while recalling K. Bühler's (4) attempt to integrate the schools by positing three fundamental points of view for every problem in psychology.

[5] Klein (7) has already eloqunetly called attention to this reason for the dissension in psychology.

[6] One might have hoped that individuals destined to become psychologists would have profited more by the lore of the nursery than to have so completely forgotten the fable of the blind men and the elephant.

[7] The particular content in which the fallacy of arrogation is clothed by different psychologists is probably determined by such factors as Russell (9, pp. 29–30) acutely calls attention to in these words: "The manner in which animals learn has been much studied in recent years, with a great deal of patient observation and experiment. Certain results have been obtained as regards the kinds of problems that have been investigated, but on general principles there is still much controversy. One may say broadly that all the animals that have been carefully observed have behaved so as to confirm the philosophy in which the observer believed before his observations began. Nay, more, they

3. With regard to *the constructive steps which are indicated*, is it possible to abandon the fallacy of arrogation and its attendant worship of the rigorous, even if prematurely closed, system without falling prey to an opportunistic eclecticism? In other words, can psychological investigation be organized in such a way that whatever systematic unity is implied in the complementary pattern of the schools will have the best chance of being realized and recognized? Bridgman's (1, 2, 3) operationism suggests itself as one possible means to this end.[8]

As a corollary to the operational point of view it is desirable

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have all displayed the national characteristics of the observer. Animals studied by Americans rush about frantically, with an incredible display of hustle and pep, and at last achieve the desired result by chance. Animals observed by Germans sit still and think, and at last evolve the solution out of their inner consciousness. To the plain man, such as the present writer, this situation is discouraging. I observe, however, that the type of problem which a man naturally sets to an animal depends upon his own philosophy, and that this probably accounts for the differences in the results. The animal responds to one type of problem in one way and to another in another; therefore the results obtained by different investigators, though different, are not incompatible. But it remains necessary to remember that no one investigator is to be trusted to give a survey of the whole field."

[8] Bridgman has formulated this point of view to accomplish for physcis what psychology seems to need even more. "We should now make it our business," he writes (1, p. 2), "to understand so thoroughly the character of our permanent mental relations to nature that another change in our attitude, such as that due to Einstein, shall be forever impossible. It was perhaps excusable that a revolution in mental attitude should occur once, because after all physics is a young science, and physicists have been very busy, but it would certainly be a reproach if such a revolution should ever prove necessary again." He then proposes (p. 5) the principle according to which this goal may be attained, namely, that "we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*" "It is evident," he continues (p. 6), "that if we adopt this point of view toward concepts, namely that the proper definition of a concept is not in terms of its properties but in terms of actual operations, we need run no danger of having to revise our attitude toward nature. For if experience is always described in terms of experience, there must always be correspondence between experience and our description of it . . . " As a matter of fact the germ of this principle of operationism first became important in the history of thought through the writings of a famous psychologist, William James. He called it pragmatism, deriving both the name and much of the theory from C. S. Pierce. It is even more intimately a psychological notion, however, for, as Bridgman says, it arises from a critique of "the character of our permanent mental relations to nature." As such it should certainly not be alien to the psychologist's natural tastes. That it is not is shown by Stevens' (10, 11) recent attempt to bring forward the principle as a panacea for all of psychology's ills. His tone, however, far too much suggests a new 'school.'

that the problems studied and the concepts employed in psychology should not arise, as they so often do at present, within the setting of a special school upon which alone their significance depends. Research so inspired is undertaken as a sort of propaganda or as part of a campaign for converts and as such it loses value as soon as the winds of doctrine have shifted. If, instead, problems were selected because of their own urgency in real life-situations and were then attacked by the best available operational methods, the importance of the work accomplished would be lasting instead of transitory.

REFERENCES

(1) Bridgman, P. W. The Logic of Modern Physics. New York: Macmillan, 1928. Pp. xiv + 228.
(2) Bridgman, P. W. The Nature of Physical Theory. Princeton: Princeton Univ. Press, 1935. Pp. 138.
(3) Bridgman, P. W. A physicist's second reaction to Mengenlehre, Scripta Mathematica, 1934, 2, 101–117; 224–234.
(4) Bühler, K. Die Krise der Psychologie. Jena: Fischer, 1927. Pp. xv + 223.
(5) French, T. M. Interrelations between psychoanalysis and the experimental work of Pavlov, Amer. J. Psychiat., 1933, 12, 1165–1203.
(6) Heidbreder, E. Seven Psychologies. New York: Century, 1933. Pp. viii + 450.
(7) Klein, D. B. Eclecticism versus system-making in psychology, Psychol. Rev., 1930, 37, 488–496.
(8) Köhler, W. Die physischen Gestalten in Ruhe und im stationären Zustand. Verlag der Philosophischen Akademie: Erlangen, 1924. Pp. xx + 263.
(9) Russell, B. Philosophy. New York: Norton, 1927. Pp. v + 307.
(10) Stevens, S. The operational basis of psychology, Amer. J. Psychol., 1935, 47, 323–330.
(11) Stevens, S. The operational definition of psychological concepts, Psychol. Rev., 1935, 42, 517–527.
(12) Wertheimer, M. On truth, Soc. Res., 1934, 1, 135–146.

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DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

Sir:

Mr. Benjamin, in his article entitled "Outlines of an Empirical Theory of Meaning" (PHILOSOPHY OF SCIENCE, July, 1936), asserts in his course of argument that tautological propositions are not unique to the field of logic, i.e. propositions which might be considered tautological can be found in every empirical universe of discourse. That the significance of the concept of tautology is dependent upon an arbitrary act, in which we can in every case invent the required definition, through the intensional route. This conception of tautology is in the case of Mr. Benjamin a direct outcome of the acceptance of a statement given by Lewis and Langford in their "Symbolic Logic": that, the tautology of any law of logic is merely a special case of the general principle that what is true by definition *cannot conceivably be false* (italics mine): it merely explicates, or follows from, a meaning which has been assigned . . . (Page 211).

At this point, may I make it my concern to state that Mr. Benjamin in his eagerness to press an issue has lost sight of a rather important element in the concept of tautology, a point which is implicit in the above quotation and which is often made explicit in other sections of the source from which the author quotes.

The phrase, "cannot conceivably be false," has a meaning which is unique to the field of logic. A proposition, "cannot conceivably be false," signifies a proposition whose assertion exhausts the possibilities, i.e. "any law of logic is equivalent to some statement which exhausts the possibilities; whatever is affirmed in logic is a truth to which no alternative is conceivable." (Ibid. Page 24), such as $p \cdot \vee \cdot \sim p$, or $p \cdot p \supset q : \supset q$. A proposition of the type; "Force is directly proportional to the product of the masses and inversely to the square of the distance," is not a

tautology, although it is the direct outcome of definition and implicit assumptions, since it may "conceivably be false" and therefore not exhaustive of the possibilities or alternatives. The history of Physics is a justification for the last assertion. Constant revision, that which we call progress, is a consequence of the establishment of any "natural law." The "laws" of logic, on the other hand, are of such a nature that we can only reject those which are found to be contradictory relative to a certain system.

Whether this criticism acutely affects Mr. Benjamin's general thesis is a question which only Mr. Benjamin can decide. I may venture to suggest that unless tautologies are shown to be common to empirical or extensional symbols, in some other fashion than that professed by the author at present, the dichotomy between extensional and suppositional symbols will still persist.

Israel C. Biederman.

The Pragmatism and Scientific Metaphysics of C. S. Peirce

A Review of Volumes V and VI of the Collected Papers of Charles Sanders Peirce edited by Charles Hartshorne and Paul Weiss, Harvard University Press

THE FIFTH volume of the *Collected Papers of C. S. Peirce*, entitled Pragmatism and Pragmaticism,[1] contains papers dealing with two distinguishable, but interconnected doctrines: Pragmatism and Critical Common-sensism. The latter, antedating in its earliest expositions[2] the first formulation of the pragmatic doctrine in 1877, 8,[3] is however later conceived by Peirce as a consequence of pragmatism.[4] The two doctrines will be advisedly treated here in isolation, and first pragmatism.

Pragmatism is a doctrine concerning the meaning of ideas, but not of all ideas. It gives a meaning only to "intellectual con-

[1] "Pragmaticism" is the name given by Peirce in later years to his own form of Pragmatism in order to distinguish it from the doctrines of other Pragmatists, with whom he was not in complete accord. See Volume V, ¶414.

[2] Peirce did not give it this name until much later (1905), but it is plainly the doctrine aimed at in Bk. II, Papers I–III (1868). See 5.439. All isolated numerical references in the review are to the numbered paragraphs of Peirce's *Collected Papers*. The whole number specifies the volume, the decimal number, the paragraph.

[3] Bk. II, Papers IV, V, on "The Fixation of Belief" and "How to Make Our Ideas Clear," from Popular Science Monthly. The Papers of Bk. II had all been published previously. Bk. I and III are published here for the first time.

[4] 5.439.

ception,"[4] *i.e.* to "those upon the structure of which arguments concerning objective fact may hinge."[5] What this excludes is ideas which are mere qualities of feeling,[6] and all proper names or other designations of individual objects.[7] Thus those thoughts or words whose meaning it elucidates are predicates,[8] and generals.[9]

Intellectual concepts "essentially carry some implication concerning the *general* behavior either of some conscious being or of some inanimate object, and so convey more, not merely than any feeling, but more too than any existential facts."[10] Pragmatism asserts, that the total meaning of the predication of an intellectual concept is contained in an affirmation that, under all conceivable circumstances of a given kind (or under this or that more or less indefinite part of the cases of their fulfillment, should the predication be modal) the subject of the predication would behave in a certain general way."[11]

The purpose of Pragmatism is to "give expeditious riddance of all ideas essentially unclear. In the second place, it ought to lend support, and help to render distinct, ideas essentially clear; but more or less difficult of apprehension."[12] More specifically, "it will serve to show that almost every proposition of ontological metaphysics is either meaningless gibberish—one word being defined by other words, and they by still others, without any real conception ever being reached—or else is downright absurd; so that all such rubbish being swept away, what will remain of philosophy will be a series of problems capable of investigation by the observational methods of the true Sciences."[13] This insistence that metaphysics is to be purified by the elimination of the meaningless and the treatment of the remainder by the method of the

[5] 5.467.
[6] *Ibid.*
[7] 5.429 ff.
[8] 5.469.
[9] 5.430 ff.
[10] 5.467.
[11] *Ibid.*
[12] 5.206.
[13] 5.423.

successful experimental sciences is a recurrent theme from first to last.[14]

For such an aim and with the pragmatic maxim as an instrument for its prosecution, the reviewer can feel nothing but praise. Two fundamental questions, however, disturbed his peace of mind after reading Volume V. Has Peirce gone far enough and does the pragmatic maxim suffice? And what would application of the pragmatic maxim do to the arguments and analyses designed by Peirce to establish that maxim? Our first consideration must be of the latter question.

Two factors are ingredient in every one of Peirce's papers: first, he is dealing with thought and belief, and secondly he conceives thought as being, or being correlated with a sign. The actual analysis turns upon an account of the nature of the signs.

Peirce's treatment of signs involves the distinction between what the sign "represents" and what it "signifies." These are closely related to extension and intension respectively. The pragmatic meaning is the signification. Only general signs have a signification and only predicates are general. The problem then is to indicate what that signification of a sign may ultimately be.

Two views are taken. Sometimes it is said that the signification is a deliberate, *i.e.* self-controlled, habit.[15] Sometimes it is said that the signification is that verbal expression, which is "most applicable to the self-control of human conduct under every situation and to every purpose," *i.e.* it is "the general description of all the phenomena which the assertion of the proposition virtually predicts."[16]

It is plain that Peirce usually and characteristically conceives a sign as a thought, seldom as a word, mark, or other manipulable external object. To the reviewer's way of thinking, this mode of conception introduces serious difficulties. In the first place, any study of thought as a human or intelligent process, would seem to be possible only on the basis of an elaborate metaphysical theory, if not even involving elements of psychological doctrine.

[14] See *e.g.* 5.412.
[15] 5.480, 491.
[16] 5.427.

Yet these Peirce explicitly wishes to avoid.[17] In the second place, it would seem necessary to *establish* the analogy between the mode of functioning of such an external sign as a word, sentence, or argument, and that of a thought. This problem is glossed over.

Still further difficulties are to be encountered. The reality of a sign involves the presence of intelligent action.[18] All right. But intelligent action is action genuinely triadic, it is action of this sort: one event, A, produces a second event B, *as a means* to the production of a third event C. The mediating event is in this case said to be a sign.[19] This analysis, by itself is not sufficient. It is necessary to Peirce's end that we declare the production of one event by another, and the fact that an event functions as a means, to be facts directly perceivable.[20] In fact, Peirce goes so far as to say that there is no conception capable of entertainment which is not first given in sense.[21] These and numerous other claims seem, to say the least, to stand in need of clarification by application to them of the pragmatic maxim.

Another point that cannot go unnoticed is Peirce's adherence to Scholastic Realism. One cannot read ten consecutive pages without being reminded of it. It is in part a consequence of Peirce's own pragmatic analysis of the meaning of *reality* and of *real*. The accounts offered in the earliest and the latest articles do not differ in any significant manner. The statement in 1878 is "The opinion which is fated to be ultimately agreed on by all who investigate is what we mean by truth, and the object represented in that opinion is the real."[22] While in 1905, he speaks of a "destined" opinion, and says that, "the state of things which will be believed in that ultimate opinion is real. But, for the most part, such opinions will be general. Consequently some general objects are real."[23]

The reviewer would not hesitate in his adherence to the doctrine

[17] 5.28, 121, 452.
[18] 5.472, 3.
[19] *Ibid.*
[20] 5.212.
[21] Cf., *e.g.*, 5.181.
[22] 5.407.
[23] 5.430.

that some general propositions are true. This claim is not, however, the distinguishing feature of Realism, for all Nominalists agree. Peirce's contribution lies in his explanation of the nature of truth and falsity, and in his correlation of propositions affirming the reality of an object with true propositions. The first of these two factors may be granted for the sake of the argument, although it is laden with difficulties. But the second must make us pause. What Peirce succeeds in doing is effecting the correlation of entire propositions one to the other. Yet the adequacy of this rule of correlation depends on our being able unambiguously to identify and describe "the state of things which will be believed in,"[24] when one believes a certain proposition true.

Or maybe the correlation is a one—many correlation so that many distinct states of things are believed in when one believes a given proposition true. By what rules do we determine whether a description or other designation of a state of things is to be correlated with a given proposition? Until this can be settled, the analysis fails to supply the illumination that it professes.

But even granting that the rule is found, and that some of these states of things correlated with true propositions are generals, so that generals may be said to be real; this still leaves largely unsettled the nature of these generals. Do they include predicates, whether of one or more than one subject?[25] And do they include other things than predicates? But predicates, as entities, are in our opinion "hypostatic abstractions."[26] This abstractive process may indeed be useful, but does this elucidate the nature of real generals. Or if they are other than predicates, are they perhaps laws?[27] But what is the difference between a real law and a true general proposition? What, then would be the difference between nominalism and realism?

We must pass on to other problems. There are three directions in which Peirce's attack on traditional conceptions of methodology does not appear to the reviewer to have gone far enough. One

[24] 5.430.
[25] Cf., *e.g.* 5.102.
[26] Cf. 5.449.
[27] Cf., *e.g.*, 5.99,105.

of these consists in Peirce's own practice, and has been more or less adequately illustrated already. One more illustration shall suffice. The elaborate *Lectures on Pragmatism* of 1903,[28] designed to exhibit Pragmatism as nothing more than a principle of logic, involve in their complex development a conception of the divisions of Philosophy and of the order of derivation of these divisions to which the reviewer cannot subscribe.[29]

First he puts Phenomenology, or the contemplation of the Universal phenomena; then the normative sciences: esthetics, ethics, and logic, in that order: finally comes Metaphysics, which aims "to comprehend the Reality of Phenomena." These problems of classification and ordering are treated by Peirce more at length in volume I of the *Collected Papers*. This is not the place in which to propose alternative classifications. It must suffice to declare that much of what Peirce throws into those divisions antecedent to Metaphysics would seem to the reviewer tenable only as consequences derived from the metaphysics. Thus for example, the Universal categories of Phenomenology, firstness, secondness and thirdness,[30] require the application of metaphysical theory and can hardly be the result of any simple perceptive act.

It occurs to us, furthermore, that metaphysics would to a large extent, consist in nothing more than the careful application of the Pragmatic Maxim to these concepts which are for Peirce pre-metaphysical. Thus, *e.g.*, we experience considerable difficulty in apprehending what is the pragmatic meaning of the synthesis in thirdness of such, to our way of thinking, distinct, albeit related, concepts as those of habit, thought, representamen, predicate, and law.

The fact, however, that such criticisms as those just made are at all possible, would seem to indicate that some yet higher grade of clearness is requisite than that to which the maxim of pragmatism will lead us. Or rather, what is required is a technique for the application of the maxim, a means of determining when it

[28] Vol. V., Bk. I.
[29] Cf. Bk. I, Lect. IV, §1; also 5.37.
[30] Lectures, II, III, IV.

shall have been applied. This in fact is what, as we see it, the maxim aims to supply for Descartes' principles of clearness and distinctness.[31] Descartes' own description of the criteria and the technique seem to supply only a fallible instrument. The reviewer suggests that while the conception of Pragmatism as the sole guiding principle of abduction[32] is illuminating this one maxim should, at the stage of pre-metaphysical logic, be replaced by a set of rigorous formal rules of inference which in their totality might be shown in the post-metaphysical stage of logic (*i.e.* at the stage of scientific epistemology) to be equivalent in effect to the one maxim of pragmatism. They would then constitute the pragmatic interpretant of the pragmatic maxim. In the pre-metaphysical stage what is required is something as unambiguous as abstract logic or Carnap's logical syntax.[33]

Thirdly, we come to the question of theoretic completeness of the doctrine. Pragmatism affirms that the meaning of a concept lies, or is given, in a "general description of all the experimental phenomena which the assertion of the proposition virtually predicts."[34] It is given in "a general conditional proposition as to the future."[35] The question is in what terms is this description of the circumstances of an experiment and the description of the predicted results of performing that experiment to be phrased? Are not the terms employed likely in many cases to stand in need of explanation by the application to them of the pragmatic maxim? And in theory at least would this not always be the case so that we should be involved in an infinite progressus? In part, Peirce recognises this difficulty and says that until we come to something different than a proposition as a meaning we shall have reached nothing for which we could not yet ask for an interpretant. And it is because of this that he locates the meaning

[31] Cf. Bk. II, Paper V, §1,2.

[32] Book I, Lecture VII, §3. "Abduction" is the name given by Peirce to that type of reasoning in which we pass from the observation of facts to the suggestion of an explanatory hypothesis that will subsequently be tested by induction. It is a type of inference and hence its guiding principles would be principles of logic. Cf. 5.171.

[33] Rudolf Carnap: *Die logische Syntax der Sprache* (1934).

[34] 5.427.

[35] 5.432.

in a habit rather than in a proposition.[36] Still, from the methodological and logical points of view, this solution is not adequate. For there we cannot get outside the system of signs, and we must ask which of the many equivalent propositions, all ultimately interpreted by the same habit, is to be taken as giving the meaning in such a sense that no other proposition need replace it as a more adequate analysis.

It is the opinion of the reviewer that at least one of the aims of phenomenology is to satisfy this requirement. Namely, it sets aside one category of predicates or propositions whose analysis by means of other propositions is not required, and it attempts to to analyze all other propositions by means of logical functions (largely conditional) that employ only propositions of the designated type or category for their atomic propositional elements. Here then we secure a precise objective criterion to determine whether or not analysis has been carried far enough: is every atomic propositional element of the analysing proposition a proposition of the designated type. But Peirce specifically rejects phenomenalism[37] without replacing it by any other doctrine. Now it is not claimed here that phenomenalism, or any other particular doctrine as to what concepts and propositions shall be construed as atomic, *i.e.* as not requiring analysis of the sort proposed by the pragmatic doctrine, should be unequivocally espoused by pragmatism. But it is affirmed that pragmatism will be a doctrine vitiated by an infinite progressus unless it recognizes that there is *some* such category of propositions, and that it is not satisfactorily applicable until some such category is actually specified.

Perhaps it was the opinion of Peirce that he avoided this objection by his espousal of what he calls Critical Common-sensism, the other major doctrine treated in Volume V. For it would seem to answer the purpose, and he views it as a consequence of pragmatism.[38] It results from the conjunctive influences of the Scotish

[36] 5.486, 491.
[37] 5.428.
[38] 5.439.

Common-sensism, Kantism, and a disaffection for Cartesianism. It is said to have six distinguishing characteristics:[39]

(1) It admits that there are not only indubitable propositions, say perceptual judgments, but indubitable inferences, like St. Augustine's "*Cogito, ergo sum.*" Both kinds are indubitable because they are acritical. Doubt follows belief, not vice versa,[40] and the dubitable is only that over which we can exercise self-control. Genuine doubt always has an external origin, like surprise, and cannot be induced by any direct act of the will.

(2) The list of original indubitables changes only slightly from generation to generation.

(3) These original beliefs are of the nature of instincts. But they remain indubitable only in their application to affairs resembling those of a primitive mode of life.

(4) The acritically indubitable is necessarily vague.

(5) Genuine doubt is highly valuable and the critical common-sensist invents a plan for attaining doubt.

(6) Critical common-sensism fairly claims its title for two reasons: (a) It subjects four opinions to criticism: its own, that of the Scotch school, that of those who would base logic on metaphysics and psychology and that of Kant. (b) It is but a modification of Kantism, only rejecting the proposition that a thing-in-itself can be conceived.

Now it might be thought that this class of indubitables constituted the category of propositions and predicates not requiring analysis by the application of the pragmatic maxim. But that this cannot be the case it is not difficult to show.

In the first place, it cannot be indubitable propositions that form this class, for what is required is rather a class of predicates some values of which will be false. Otherwise the logical constructions employing them as atomic bases and intended as analyses of other predicates would have each a constant truth-value determined merely by its form.

Nor can they be the predicates that enter into these indubitable

[39] Bk. II, Paper VII, §1.
[40] 5.512.

propositions. For these predicates are said to be vague and the truth or falsity of their values to be indubitable only within a restricted range—*i.e.* where the value makes application to a state of affairs resembling those of a primitive mode of life. In order to determine the truth or falsity of applications beyond this limited sphere, an analysis of the predicate in question must be made.

Despite this fact, a scientific use will be made of these indubitables. They constitute a part of what the reviewer has elsewhere designated as the "social given."[41] Its value lies in the measure of direction that it may impart to the first frantic gropings of a speculator become conscious of the importance of imagination and of the boundlessness of its freedom. Scientific thought cannot simply perform experiments and report "it is true" or "it is false." There must be some concept with which it approaches the task of experimentation and the limits of whose application the experiment will indicate. But after all, what we want is not merely to find out whether any old concept is true or not, we want to find some that are true. What shall we select? The social given operates to limit the field of choice to manageable dimensions, by the analysis of its predicates, which the presence of the social given assures us are true within a limited range, we find a starting point for our search for predicates worth the trouble of analysis and experimental testing.

A precisely similar use is made of the indubitable inferences. They constitute a field of fact whereby we may experimentally verify an analysis of validity proposed in some set of explicit logical principles of inference.[42]

The extent to which the preceding pages have been devoted to criticism might reasonably lead the reader to suspect that nothing to the reviewer's liking was to be found in Volume V. But quite the contrary is in truth the fact. Peirce's treatment of induction, and of probability, his discussions of Kantism and his deviations therefrom, as well as his discussions of Cartesianism, were illum-

[41] See his essay on "Logical Positivism and Speculative Philosophy" in *Philosophical Essays for Alfred North Whitehead* (Longmans, 1936), §18. See also §12 and §13.

[42] Cf. *Ibid.*, 13, 15.

inating and persuasive. And other incidental developments too numerous to mention thoroughly repay the reader of the volume. His account of the nature of signs is important, but suffers from obscurity and is vitiated, as previously noted, by premature attempts to treat physical symbols and thought under one heading and simultaneously. For his development of the pragmatic theory and for the influence which it has had upon his successors there can be nothing but wholehearted appreciation.

In Volume VI, on Scientific Metaphysics, one might expect to find the pragmatic principle at last being actually put to use. One does find here what the inventor of pragmatism claimed as its chief service, a surprising diminution in the number of problems that are treated. But this is to be accounted for only in part by the influence of pragmatism. Many problems have disappeared in virtue of their having been relegated to other, pre-metaphysical, branches of philosophy. This feature has been already commented on.

On the other side, the pragmatic, or experimental, method does not seem to be the sole guiding principle determining the development of theory in this volume. Most briefly put, the object of the pragmatic method is to eliminate surprise. It accomplishes, or aims to accomplish this end by the elaboration of explanatory hypotheses which, having been experimentally verified, enable us to predict phenomena. Characteristically, this explanation takes the form of a general law. It is at this point that the other guiding principle enters in to affect the development of theory. According to Peirce, the fact that any particular law should be true is itself a surprising fact. Experimentation shows it to be true. But this fact of its truth is a new fact wanting explanation. And in order to explain it, we must propose another, higher, hypothesis.[43] The presence and the extent of regularity are facts to be explained, as also is the fact of the diversity upon which this regularity acts, or to which it applies. What we need then are ultimately hypotheses experimentally verified and not themselves requiring explanation. It is this second requirement that enters as a non-pragmatic directing principle.

[43] Vol. VI, Bk. I, ch. 2, §4.

Peirce is constrained in the light of it to adopt the hypothesis that absolute chance is a real factor in the world and that chance is not merely a reflection of human ignorance.[44] The reality of chance does not stand in need of explanation, since explanation is the subsumption of a fact under a law and the doctrine in question is precisely one to the effect that law is not original. Regularity requires explanation, which it can get only by reference to something else.

All of Peirce's fundamental hypotheses are subjected to this double test, by experimental verification of their truth and by consideration of the question whether or not they require explanation themselves. The postulate of absolute chance is not the only ultimate theory upon which Peirce relies. The other is what he calls the "law of mind."[45] It is the law of habit formation, it describes the development of general ideas and of regularity.

Space will not permit so detailed an account of the contents of this sixth volume as we gave to Volume V. We shall have to content ourselves with a bare enumeration of some of the outstanding features and a general comment. The doctrines introduced, however, are not many of them novel in the sense that most of them have been treated at length by Peirce in the writings of the earlier volumes of this set.

Peirce's position is monistic, this principle recommending itself as one which, in accordance with the dictum of Occam's Razor, must be examined and found inadequate before one is justified in the adoption of a pluralistic hypothesis. The position is Idealistic and regards matter as atrophied mind, *i.e.* as a sphere within which the spontaneity of mind has been overpowered by the weight of habit. These habits constitute the laws of nature. The position is synechistic, insists on the continuity of the fundamental stuff, which is feeling, firstness. The now familiar categories of firstness, secondness and thirdness play an important part in the whole scheme. The transience of law, habit, is insisted upon. The importance of sympathy, love, is emphasized in the

[44] *Ibid.*
[45] *Ibid.*, ch. 5.

account given of evolution, and the doctrine of evolution is extended with respect to its scope.

Quite in contrast with the contents of the first book is that of the second, dealing with religion. The first aims to be scientific, precise. The second insists upon the truth of vague concepts and upon their superiority to precise ones. It further remarks that these concepts are essentially incapable of precision and are at the same time incapable of repudiation or doubt. It strikes the reader as an outstanding direct application of the principles of Peirce's critical common-sensism already discussed.

Speculation of the sort that engages Peirce in his scientific metaphysics strikes the reader as highly interesting, frequently showing evidences of brilliance, worthwhile both as an enquiry and a subject of study, but premature from the point of view of genuine science. It is speculation with a certain amount of corroborative evidence. But it is too general to find justification at this stage of scientific enquiry. Furthermore, in its ascent to the second level of generality, it tends to lose sight of the genuinely scientific attitude. The generality required from the scientific viewpoint would be that of a wider law capable of specification; that proposed is a generality which views scientific laws as instances rather than subspecies. This elevation of type is not required here by any considerations of scientific procedure, and would from the reviewer's point of view better be abandoned.

One cannot close without remarking on the vast number of startling similarities that are to be found when one compares the work of Peirce with that of Whitehead. Certain differences of style and of method are outstanding. But the number of the common doctrines, both general and special, is beyond what might be expected. Both thinkers break into new paths of thought, but it largely seems like the common exploration of one new path.

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A NEW BUDGET OF PARADOXES

(7) NOTES FROM A CASE-BOOK

Case 10. Emulsion-maker. From Singapore comes the "embryo of an ideal sociology" resulting from the author's visits to 60 countries, "which will enable people to get their noses off the grindstone without getting them under the tombstone." He needs an island in the Pacific, although "there are many magnificent cities under the sea." The labor question is solved by the use of monkeys as workmen and cormorants for catching fish. Of course, there are 10 new commandments, and a prescribed daily "baptism" with soap and water. Fitness of colonists is made to depend on psychometry, phrenology and clairvoyance, but "any kind of a muff can be king." Some good tips are to be derived from a lecture on Mars. There the people are taller, very uniform, and more spiritual. They barter without coinage, wear simple clothes, pursue small scale agriculture. They converse only by thought transfer, wrestle for exercise, *sit* at prayer, disdain kings but do choose men of wisdom to rule over little communes of 500 each. Testimonials are cited from Andrew Jackson Davis, the Poughkeepsie seer, Galen, Swedenborg and Dr. T. Young, a genius who was born of "ordinary Quaka patents." (Sic in Singaporese!).

Case 11. Phantom-crier, alarmist. Suggestions as to the Necessity for a Revolution in Philosophy by Q. E. D. written in 1848. This geometrizing anonymity is all heated up about the formation of a "Mental Fever Society."

Case 12. Magician, illusionist. In 1847 Laban Alverson held forth in Ann Arbor, Mich. in a series of lectures by the clairvoyant. He seems to have found a key to magnetism whereby he unlocked the secret of the settlement of America both before and after the Deluge. At the same time, however, he had discovered, so-to-speak, an *atomic theory* of the solar system, because he claimed that positive electricity keeps the planets in their courses.

Case 13. Overman, god. A contribution from Salt Lake City is devoted to the discovery of the soul and the law of its development. The Creative Order is given in some detail, culminating in a eulogy to the Great Pyramid. The "Law of Consequences" is a combination of Newton's third law and "whatsoever a man soweth that shall he also reap." The secret is entirely given away in the words: "What I am above I myself have been, else I were not above; for what I am above only because I have been. And whereas I have been what I am above, I still am what I am above, nothing wanting, nothing lost; but all required to constitute what I am."

W. M. M.

SUPPLEMENT
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Foundations of Probability in Mathematical Logic[1]

BY

THEODORE HAILPERIN

IT IS the purpose of this paper to present a theory of probability derived from two-valued logic—the logic of which an aspect is given in Part I, Section A, of *Principia Mathematica*. The symbolic system of Mr. Keynes, given in his *Treatise on Probability*, will be shown to be a part of our system. We have, however, little if anything in common with his philosophical analysis; a definition of Keynes' fundamental probability relation, free from psychological or material reference, will be given, enabling us to offset some of the objections to his theory.

I. THE TRUTH-FUNCTION

Although Wittgenstein's construction of the truth-functions is well known certain additions are to be introduced; hence it is important to outline this method in full.

[1] I wish to express my thanks here to Dr. Ernest Nagel of Columbia University for his generous counsel and assistance in the development of this theory.

We first introduce the tables:

p	$\sim p$
T	F
F	T

(A)

p	q	$p \vee q$	$p \cdot q$	$p \supset q$	$p \equiv q$
T	T	T	T	T	T
T	F	T	F	F	F
F	T	T	F	T	F
F	F	F	F	T	T

(B)

Table A indicates that the proposition "not-p" ($\sim p$) has the truth-value F (falsity) when p has the value T (truth), and has the value T when p has the value F. Similarly in B "p or q" ($p \vee q$) has the value T unless both p and q are F; "p and q" ($p \cdot q$) is true only when both p and q are; and so on for implication ($p \supset q$) and equivalence ($p \equiv q$).

A combination of propositions and logical constants (and, or, implication, etc.) such that its truth-value is determined when that of each of its constituent propositions is, is called a *truth-function*. A truth-function which has always the value T no matter what values are assigned to the propositions is called a tautology. An example:

p	$\sim p$	$p \vee \sim p$
T	F	T
F	T	T

This shows that "p or not-p" is a tautology since in the third column appear all T's. A similar array is obtained for "p implies p or q" and all other propositions of pure logic—(an abundance examples will be found in *Principia Mathematica*, Part I, Sec. A). The negations of these propositions, which always have the value F, are called *contradictions*.

Just as the proposition "p or not-p" was formed by affixing the value T to the two combinations of truth-values (called truth-possibilities) under $\underline{|\ p\ |\ \sim p\ |}$, so any truth-function can be obtained by assigning appropriate values to matrices of truth-possibilities such as

p
$\mathcal{T}$
F

I

p	q
$\mathcal{T}$	$\mathcal{T}$
$\mathcal{T}$	F
F	$\mathcal{T}$
F	F

II

p	q	r
$\mathcal{T}$	F	F
F	$\mathcal{T}$	F
F	F	$\mathcal{T}$

III

p
$\mathcal{T}$

IV

Thus from I we can form, say, the propositions

"

p	
$\mathcal{T}$	$\mathcal{T}$
F	F

" and "

p	
$\mathcal{T}$	F
F	$\mathcal{T}$

",

which are obviously each others negation. We can set this observation down as a rule: *the negation of a truth-function is obtained by replacing the $\mathcal{T}$ values (in the open boxes) by F's and vice versa.* This is an extension of the idea expressed in (A) for from that we can only find the negation of a truth-function which has only one possibility, $\mathcal{T}$ or F.

From II we can derive propositions as

"

p	q	
$\mathcal{T}$	$\mathcal{T}$	$\mathcal{T}$
$\mathcal{T}$	F	$\mathcal{T}$
F	$\mathcal{T}$	$\mathcal{T}$
F	F	F

"

(C)

The question arises Can (C) be expressed as a truth-function of other propositions? Can the concepts of disjunction, implication, etc. be applied to such propositions?

Before answering this question it may be pointed out that some logicians and mathematicians have not considered these constructions such as (C) and the two formed from I as propositions. Weyl,[2] for example, saw in (C) merely a rule for finding the truth-value of "p or q" given the values of p and q (cf. under $p \vee q$ in

[2] Rice Inst. Pamph. Vol. XVI, no. 4. In this he is following the Hilbert school.

B). However if we can apply the notions of the logical constants to them I see no reason for refusing to admit them as propositions in good standing.

With great acumen Wittgenstein perceived that (C) could be considered as a proposition; he called it "p or q." This is quite all right if we remember that p and q are here limited to propositions which have possibilities of the form

p
$\mathcal{T}$
F

Some propositions can't have these possibilities; for instance, a tautology can only have the truth-possibility $\mathcal{T}$ (vide form IV above). It is best therefore to indicate the truth-functional form of p and q; for this reason and for purposes of generality we write (C) as

"

p	
$\mathcal{T}$	$\mathcal{T}$
F	F

or

q	
$\mathcal{T}$	$\mathcal{T}$
F	F

"

To show that this is permissible we must extend, or rather, *generalize* the notion of constructing truth-possibilities. The first two columns in table B show that the truth-possibilities of two propositions which have only one truth-value, $\mathcal{T}$ or F, are found by forming all the possible combinations of $\mathcal{T}$ and F. The extension to propositions which have more than one value alongside the truth-possibility matrix is obvious.

Rule. The truth-possibilities of two truth-functions are obtained by forming all the possible combinations in pairs of their truth-values, taking the first value from the first set of values and the second from the second set (i.e. the set of values assigned to the possibilities).

A similar rule can be given for finding the possibilities of n propositions.

Clearly, if corresponding to a value of the first set there are n possible values of the second set then the first value is to be written down n times. A few examples will make this familiar.

The truth-possibilities of

p
$\mathcal{T}$
F

$\mathcal{T}$
F

$(= p')$ and

q
$\mathcal{T}$
F

$\mathcal{T}$
F

$(= q')$

are

p	q
$\mathcal{T}$	$\mathcal{T}$
$\mathcal{T}$	F
F	$\mathcal{T}$
F	F

(α over the first two rows, β over the last two rows)

for when p' has the value $\mathcal{T}$, q' has the possible values $\mathcal{T}$ and F (assuming no truth-functional dependence between p and q, in order to avoid complications now); this is represented in the bracketed part marked α. Similarly when p' has the value F, q' again has the possible values $\mathcal{T}$ and F; this is shown in β. Obviously to find the disjunction "p or q" we would simply take the disjunction of the values under p and q in accordance with table (B) at the beginning of this section. Performing this operation we would get (C)—the result which we have anticipated.

For a second example let us use the propositions

p
$\mathcal{T}$
F

$\mathcal{T}$
F

$(= a)$ and

p	q	
$\mathcal{T}$	$\mathcal{T}$	$\mathcal{T}$
$\mathcal{T}$	F	F
F	$\mathcal{T}$	F
F	F	F

$(= b)$

(The reader can easily verify that this second proposition b is the conjunction of p' and q' above.) The truth-possibilities of a and b are:

a	b
$\mathcal{T}$	$\mathcal{T}$
$\mathcal{T}$	F
F	F
F	F

(α over the first two rows, β over the last two rows)

for whan a has the value $\mathcal{T}$ (which means p has the value $\mathcal{T}$) then b can have the possible values $\mathcal{T}$ and F; for when p has the value $\mathcal{T}$, b has the values $\mathcal{T}$ and F. This gives us the possibilities α. When a has the value F, i.e. p has F, then b has the values F and F. This gives us β.

This application of logical notions to propositions which have sets of truth-values obviously necessitates a change in the definition of a truth-function from "a combination of propositions such that its *truth-value* is determined . . . . " to "a combination of propositions whose *set of truth-values* is determined when those of its constituent propositions are." The rule which we have given above and illustrated suffices to determine this set of values given those of the propositions which enter into the function.

A tautology has always the value $\mathcal{T}$ when any truth-functions are inserted for the propositions. This fact makes it unnecessary to indicate the truth-values of the propositions occurring in the tautology—even though the sets of values of p and q are not indicated, we know that the values of, for example, $p \cdot \supset \cdot p \vee q$ are always $\mathcal{T}$, whereas "p or q" has no definite set of values if p and q are not assigned definite sets

The forms III and IV above, although an innovation, should be no cause for difficulty; III is the matrix of possibilities of three propositions such that one, and no more than one, is true. Thus possibilities such as

$$\boxed{\mathcal{T}\,|\,\mathcal{T}\,|\,F} \quad \text{and} \quad \boxed{F\,|\,F\,|\,F}$$

are excluded, just as the possibilities

$$\boxed{\mathcal{T}\,|\,\mathcal{T}} \quad \text{and} \quad \boxed{F\,|\,F}$$

are ruled out for

$$\boxed{p\,|\,\sim p}$$

By giving definining conditions for propositions, truth-functions of any number of possibilities (and thus of values) can be obtained.

The following is a list of tautologies which will be required in

subsequent sections. No proofs will be given for they can be easily tested by means of the tables (A) and (B).

1.1 $p \cdot p : \equiv \cdot p$

1.2 $\sim(p \cdot \sim p)$

1.3 $p \cdot \supset \therefore p \cdot q : \equiv \cdot q$

1.4 $p \vee q : \sim p \cdot q \therefore \equiv : \sim p \cdot q$

1.5 $p \vee q \cdot p \vee \sim q : \equiv \cdot p$

1.6 $p \vee q \cdot \vee \cdot \sim(p \,.\, q)$

1.7 $p \equiv q \cdot \supset : p \cdot \equiv \cdot p \vee q \cdot \equiv \cdot p \cdot q$

1.8 $p \supset q \cdot \supset : p \vee q \cdot \equiv \cdot q$

The principle of the syllogism will be used implicitly, usually in the form

$$p \supset q \cdot q \supset r \cdot \supset : p \supset r \text{ or } p \equiv q \cdot q \equiv r \cdot \supset : p \equiv r$$

2. Fundamental theorems

No doubt the reader has already surmised that the proposition whose set of truth-values does not contain all T's or all F's is a possible proposition. Evidently a proposition with a proportionately greater number of T's is more probable or more likely to be certain (tautologous) since with increased knowledge some possibilities will be ruled out and the proposition with the greater number of T's will be more likely to have all T's left than one with a less number. Thus in the propositions

p	q	
T	T	T
T	F	T
F	T	T
F	F	F

A

p	q	
T	T	T
T	F	F
F	T	F
F	F	F

B

(of which A has a greater probability than B[3]) if we were given the further data that p and q cannot both be false then the possibility

F	F

[3] This does *not* say that for any propositions p and q the probability of "p or q" is greater than that of "p and q"; see 2.32 below. It will be recalled that A and B represent p v q and $p \cdot q$ only when p and q are special kinds of propositions.

would be excluded; whence A would have all $\mathcal{T}$'s (would have a probability of certainty) while B would still be less than certainty.

As we have previously mentioned, it is not the number of $\mathcal{T}$'s but the proportion of $\mathcal{T}$'s of a proposition which is germane to its probability. Accordingly the simplest definition of the probability of a proposition, which assigns certainty one and impossibility zero, is the following:

The probability of a proposition p being true—which we represent by P(p)—is the ratio of the number of $\mathcal{T}$ truth-values to the total number of values in its set of truth-values.

The probability of a proposition being false is the ratio of the number of F's to the total number of values; this is evidently the same as the probability of not-p being true. Hereafter, for convenience, when we refer to the probability of a proposition we shall mean the probability of its being true.

The idea that every proposition has an *a priori* probability associated with it, as is the case according to our definition, is *prima facie* quite extraordinary. This is not due, however, to some mystical property of divine fiat but simply a result of the truth-functional form of the proposition. If we know that a proposition is equivalent to the disjunction of two propositions, of which one must be true, then it is obvious that the proposition has a probability of certainty. Similarly the probability of any proposition depends upon the propositions into which it is analyzed. Theoretically then, to find the probability of a proposition we need but analyze it into its component propositions and these further until we come to propositions which cannot be analyzed into the conjunction, disjunction, equivalence, etc. of other propositions. The proposition will then be of the form

$$f(p, q, r \cdots)$$

where f is some truth-function and p, q, r are unanalyzable propositions. It is not difficult to see that these must be of the form

p	
$\mathcal{T}$	$\mathcal{T}$
F	F

i.e. propositions which have a probability of $\frac{1}{2}$ and are, as we shall soon see, independent. It follows that the numerical value of $f(p, q, r \cdots)$ is definite and determinable although practically this procedure of finding the probability of a proposition is hardly ever applicable.

The probability of a tautology is 1 since the number of $\mathcal{T}$'s is the same as the total number of values; the probability of a contradiction is 0 since there are no $\mathcal{T}$'s. We also have[4]

2.1 $$P(p) \leqq 1 : P(p) \geqq 0$$

Thus all probabilities lie between zero and one. Obviously the arithmetic of probabilities is the arithmetic of the positive rational numbers from 0 to 1 inclusive.

2.2 $$P(p \cdot q) + P(p \cdot \sim q) = P(p)$$

2.21 $$P(p) + P(\sim p) = 1 \qquad [2.2]$$

This proposition is obtained from 2.2 by substituting a tautology for p and using 1.3—this gives us the result that the probability *for* a proposition plus the probability *against* it is equal to one.

2.22 $$P(p \equiv q) = 1 \cdot \supset \cdot P(p) = P(q)$$

2.3 $$P(p) + P(q) = P(p \vee q) + P(p \cdot q)$$

This is one of the principal theorems in probability. The proof runs as follows:

$$\left[2.3\ \frac{p \vee q,\ \sim p \cdot q}{p \quad q}\right] P(p \vee q) = P(p \vee q : \sim p \cdot q) + P(p \vee q \cdot p \vee \sim q) \ldots (1)$$

$$[(1.4)\ (2.22)]\ P(p \vee q : \sim p \cdot q) = P(\sim p \cdot q) \ldots (2)$$

$$[(1.5)\ (2.22)]\ P(p \vee q \cdot p > \sim q) = P(p) \ldots (3)$$

$$[(1)\ (2)\ (3)]\ P(p \vee q) = P(\sim p \cdot q) + P(p) \ldots (4)$$

$$\left[2.2\ \frac{p, q}{q \quad p}\right] P(\sim p \cdot q) = P(q) - P(p \cdot q) \ldots (5)$$

$$[(4)\ (5)] \quad P(p \vee q) = P(p) + P(q) - P(p \cdot q)$$

[4] As a rule, only for less evident propositions will proofs be given. The propositions preceded by numbers are to be understood as valid for *any* propositions.

An immediate corollary is the well known

2.31 $P(p \cdot q) = 0 \cdot \supset \cdot P(p \vee q) = P(p) + P(q)$

2.32 $P(p \vee q) \geqq P(p \cdot q)$

Dem.

[2.2] $2P(p \cdot q) = 2P(p) - 2(p \cdot \sim q)$ .....(1)

[(1) (2.3)] $P(p \vee q) + P(p \cdot q) - 2P(p \cdot q) = 2P(p \cdot \sim q) + P(q) - P(p)$..(2)

[2.2] $P(p \cdot \sim q) - P(p) = - P(p \cdot q)$ .....(3)

[(2) (3)] $P(p \vee q) - P(p \cdot q) = P(p \cdot \sim q) + P(q) - P(p \cdot q)$ .....(4)

$\left[2.2 \ \frac{p, q}{q \ p}\right] P(q) - P(p \cdot q) = P(\sim p \cdot q)$ .....(5)

[(4) (5)] $P(p \vee q) - P(p \cdot q) = P(p \cdot \sim q) + P(\sim p \cdot q)$ .....(6)

[(6) (2.1)] $P(p \vee q) - P(p \cdot q) \geqq 0$

2.33 $P(p \cdot q) \leqq P(p)$ [(2.2) (2.1)]

This proposition is essentially the logical basis for Occam's Razor.

2.331 $P(p \vee q) \geqq P(p)$

Dem.

$\left[2.33 \ \frac{\sim p, \sim q}{p \ q}\right]$ $P(\sim p \cdot \sim q) \leqq P(\sim p)$ .....(1)

[(2.21)(1)] $1 - P(p \vee q) \leqq 1 - P(p)$

i.e. $- P(p \vee q) \leqq - P(p)$

or $P(p \vee q) \geqq P(p)$

2.332 $P(p) = 0 \cdot \supset \cdot P(p \cdot q) = 0$ [(2.23) (2.1)]

2.333 $P(p) = 1 \cdot \supset \cdot P(p \vee q) = 1$ [(2.31) (2.1)]

2.4 $P(p \supset q) = 1 \cdot \supset \cdot P(p) \leqq P(q)$ [(2.331) (1.8)]

2.41 $P(p \supset q) = 1 \cdot \supset : P(p) = 1 \cdot \supset \cdot P(q) = 1$ [(2.4) (2.1)]

2.421 $P(p \equiv q) = 1 \cdot \supset \cdot P(p) = P(p \cdot q) = P(p \vee q)$ [(2.22) (1.7)

An illustration for this proposition is afforded us by the "Life on Mars" paradox.

According to the classical (Laplacian) theory of probability, since there is no evidence for or against, the probability that there are horses as well as the probability that there are no horses on Mars is $\frac{1}{2}$. Similarly the probability that there are no cows, dogs, etc, is each $\frac{1}{2}$. The probability that there are no horses, *and* no cows *and* no dogs *and* etc, is $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \ldots$; and if we take a sufficient amount of live stock the probability that there is no form of life on Mars approaches zero; consequently the probability that there is some form of life on Mars approaches certainty.

This reasoning is fallacious because (among other reasons) the propositions are not independent. We shall have to defer our full discussion of independence until section 3 but it is seen that the propositions that there are no horses, no cows, etc, are all practically equivalent, so that by 2.421 their conjunction has about the same probability as any one of them and does not approach zero unless the propositions are nearly impossible.

There does not seem to be a simple rule for finding the probability of a truth-function given the probability of its constituent propositions. This difficulty can be illustrated by the fact that even if the probabilities of p and q were equal it clearly does not generally follow that those of $p \vee r$ and $q \vee r$ will be equal; for the number of truth-values in these two disjunctions depends not only on the number of T and F values in p and q but also on their truth-functional construction. Thus if p were some truth-function of r and if q were not, even though their probabilities were equal, the probabilities of $p \vee r$ and $q \vee r$ would be unequal. However in the case of propositions between which there is no truth-functional dependence it is possible to derive a general rule for finding the probability of a truth-function formed from them.

We shall say that p and q are *independent* if, in forming their truth-possibilities according to the rule in section 1, every truth-value of the one is a possible value with respect to each truth-value of the other. Thus we shall not have to take into consid-

eration the truth-functional-form of independent propositions; knowing their probabilities will suffice to determine the probability of any truth-function formed from them.

Let a be the number of $\mathcal{T}$ values of p and b the total number of its values; that is,

$$P(p) = \frac{a}{b};$$

and let

$$P(q) = \frac{c}{d},$$

then, providing p and q are independent,

$$P(p \vee q) = \frac{ad + (b - a)c}{bd} \quad \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

$$P(p \supset q) = \frac{(b - a)d + ac}{bd} \quad \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

$$P(p \cdot q) = \frac{ac}{bd} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

etc.

Let us first take (1). The number of $\mathcal{T}$'s in $p \vee q$ will be equal to the number of $\mathcal{T}$'s in p times the total number of values of q, plus the remainder (the number of F's) times the number of $\mathcal{T}$'s of q; the total number of values will be the product of the number of values of p and of q. The probability of $p \vee q$ will be the ratio of these two numbers as shown in (1). In the same manner (2) and (3) can be derived. The third equation yields

$$P(p \cdot q) = P(p) \cdot P(q)$$

if p and q are independent the probability of "p and q" is equal to the probability of p times that of q. Obviously theorems about independent propositions can be proven by simple algebraic manipulation.

The notion of independence will be taken up again in section 3 (b) where a formal definition will be given.

The concepts of possibility, consistency, etc., can be introduced by means of an interesting application of our theory in translating the system of Strict Implication given by Professor C. I. Lewis, originally in his *Survey of Symbolic Logic* and then in *Symbolic Logic* (Lewis and Langford).

In constructing a logic which he considers closer to our ordinary concepts, Lewis adds to the two truth-values (truth and falsity) a third, "p is possible." Instead of material implication (defined in section 1 herein) which is merely "it is false that p is true and q false" he defines "p strictly implies q" as meaning "it is impossible that p is true and q false," i.e. $\sim \Diamond (p \sim q)$ where the sign "$\Diamond$" stands for possibility. The axioms or primitive propositions to make these operative, of which Professor Lewis gives two different sets, are as follows (*Symbolic Logic*, Appendix).

The sign "$-\prec$" means "implies strictly"

$A1\ pq-\prec qp$

$A2\ qp-\prec p$

$A3\ p-\prec pp$

$A4\ p(qr)-\prec q(pr)$

$A5\ p-\prec\sim(\sim p)$

$A6\ p-\prec q\cdot q-\prec r:-\prec\cdot p-\prec r$

$A7\ \sim\Diamond p-\prec\sim p$

$A8\ p-\prec q\cdot-\prec\cdot\sim\Diamond q-\prec\sim\Diamond p$

$B1\ pq-\prec qp$

$B2\ pq-\prec p$

$B3\ p-\prec pp$

$B4\ (pq)\sim-\prec p(qr)$

$B5\ p-\prec\sim(\sim p)$

$B6\ p-\prec q\cdot q-\prec r:-\prec\cdot p-\prec r$

$B7\ p\cdot p-\prec q:-\prec q$

$B8\ \Diamond pq-\prec\Diamond p$

$B9\ (\exists pq):\sim(p-\prec q):\sim(p-\prec\sim q)$

Since only a contradiction is impossible we define

$$p \text{ is possible } \cdot = \cdot \Diamond p \cdot = \cdot P(p) > 0$$

it follows,

$$p \text{ is impossible } \cdot = \cdot \sim\Diamond p \cdot = \cdot \sim P(p) > 0 \cdot = \cdot P(p) = 0$$

Consequently, $p \prec q$, which is "it is impossible that p is true and q false" becomes $P(p \cdot \sim q) = 0$. Translating thus Lewis' axioms A1–8 and B1–9 we obtain true propositions. Hence

Professor Lewis' system of strict implication is merely a subsystem of ours. (It seems hardly necessary to give a detailed verification of this.)

A proposition is improbable if its probability of being true is less than its probability of being false:

$$p \text{ is improbable } \cdot = \cdot P(p) < P(\sim p)$$

and p and q are *consistent* is translated as "p and q are possible" $P(p \cdot q) > 0$. This is the "poq" of Lewis' system.

All these notions of consistency, possibility, etc., permit of an extensive elaboration of theorems the derivation of which, however, would take us from the purpose of this paper.

3. THE RELATION OF "p GIVEN q"

(*a*) *The definition*

We come now to what is perhaps the most important idea in probability. This is the relation of "p on the data of q" which we symbolize by p/q. The probability of p/q is Mr. Keynes' "a/h". Our analysis of $P(p/q)$ is quite unrelated to that of Keynes but we have taken the liberty of calling it his[5] "a/h" because it verifies all of his axioms and those "definitions" which are really disguised axioms (that is, all axioms with the exception of a certain one which requires an hypothesis).

p/q indicates that q is to be taken as data or true; hence if p is equivalent to some truth-function of q, p/q will be equivalent to that truth-function with a tautology substituted for q:

$$p/q \cdot \equiv \cdot ft$$

where f is a function such that $fq \equiv p$ and t stands for a tautology (any one will do). The probability of p on the data of q is simply $P(ft)$. That is to say, the probability of p on the data of q is the probability of a proposition, namely, the proposition which results from p by replacing q in p by t.[6]

[5] Also of Jeffries and Wrinch (Jeffries, *Scientific Inference*, p. 15n).

[6] It is always the case that there is a function f such that $fq \equiv p$, for if p and q are any propositions in general we always have

$$p \cdot \equiv : q \vee \sim q \cdot p$$

It is to be remembered that p/q is not a truth-function of p and q and that before we can apply the "matrix method" to propositions containing p/q it must be changed to ft.

Thus with our definition of the probability of a proposition this notion becomes explicit and precise in contrast to Keynes' indefinite and ambiguous "degree of rational belief". To me a belief is rational if it has a high probability!

We continue with some propositions involving the concept of the probability-relation.

3.1 $$P(p/p) = 1$$

3.11 $$P(\sim p/p) = 0$$

3.1.11 state that the probability of p given p is certainty and the probability of not-p on the data of p is impossibility. The proof is simple. Since $p \equiv (p)$, substituting t for p in (p) we have 3.1. Similarly substituting t for p in $\sim (p)$ gives 3.11.

3.2 $$P(t/p) = 1$$

3.21 $$P(p/q \cdot t) = P(p/q)$$

3.22 $$P(p/t) = P(p)$$

These propositions are all easily proven. Proposition 3.2 which states that the probability of a tautology on any data is certainty, is the form of the axioms which Mr. Keynes lists under (iii) p. 135, *Treatise of Probability*.

3.3 $$p \equiv q \cdot \equiv : p/h \cdot \equiv \cdot q/h$$

3.4 $$p/h \cdot \vee \cdot q/h : \equiv \cdot (p \vee q)/h$$

Dem. Select functions such that $p \equiv fh$, $q \equiv \varphi h$ and $p \vee q \cdot \equiv \cdot \psi h$. Substituting for p and q in the third equivalence we have

$$fh \cdot \vee \cdot \varphi h : \equiv \cdot \psi h$$

and substituting t for h we have 3.4. Similarly we have

3.41 $$p/h \cdot q/h : \equiv \cdot (p \cdot q)/h$$

3.42 $$P(p/h) + P(q/h) = P(p \vee q/h) + P(p \cdot q/h)$$

This proposition is obtained from 2.3 by replacing p and q by p/h and q/h and using 3.4.41. This is prop. (24) p. 144 in Keynes' *Treatise*. His "definition" of addition (which should be an axiom) on p. 135 is

3·43 $P(p\cdot q/h) + P(p\cdot\sim q/h) = P(p/h)$ [(2.2)]

3·44 $(p/q)/h\cdot\equiv:p/(q\cdot h)$

Dem. Let

$$(1)\quad fh\cdot\equiv\cdot p/q$$

$$(2)\quad \varphi(q\cdot h)\cdot\equiv\cdot p$$

and we are to prove $\varphi t \equiv ft$. Substituting for p in (1) we have

$$\varphi(q\cdot h)/q\cdot\equiv\cdot fh$$

But it is easily proven that the left hand member is equivalent to φh; hence $\varphi h \equiv fh$ and replacing h by t we have our result.

The following are lemmas.

3·5 $(p/q)\cdot q:\equiv\cdot p\cdot q$

Dem. Select f such that $fq \equiv p$, and we are to prove

$$ft\cdot q:\equiv\cdot fq\cdot q$$

But this is true when q is either true or false; consequently it is a tautology.

3·6 $P(p\supset q) = 1\cdot\supset\cdot P(q/p) = 1$

Dem. Let $q \equiv fp$, then

$$p\supset q\cdot\supset\cdot p\supset fp \qquad \ldots(1)$$

but

$$p\supset fp\cdot\supset\cdot t\supset ft$$

$$\supset\cdot ft \qquad \ldots(2)$$

(1)(2) $p\supset q\cdot\supset\cdot ft$

whence by 2.41 the result follows: if q is a logical consequence of p, the probability of q on the data of p is certainty.

3·61 $P(p\supset\sim q) = 1\cdot\supset\cdot P(q/p) = 0$ [3.6]

(b) Irrelevance and independence

According to Keynes p is irrelevant to q if the addition of p as data leaves q unchanged; that is, if $P(q/p) = P(q)$. We shall also take this as our definition of irrelevance.

3.701 $p \; Irr \; q \cdot = \cdot P(q/p) = P(q)$ *Df*

3.71 $t \; Irr \; p$ [3.22]

Thus a tautology is irrelevant to every proposition. ("The tautology says nothing of reality").

3.72 $q \; Irr \; p/q$

Dem. By 3.4 $(p/q)/q \cdot \equiv \cdot p/(q \cdot q) \cdot \equiv \cdot p/q$. Whence $P[(p/q)/q] = P(p/q)$. This proposition is a lemma for 3.91.

3.801 $p \; Ind \; q \cdot = \cdot p \; Irr \; q : q \; Irr \; p$ *Df*

We define "p is independent of q" as "p is irrelevant to q and q is irrelevant to p." This concept of independence is precisely equivalent to that described (*not* defined) in section 2. To prove this select two independent propositions p and q (independent in the previous sense, of course). We are to prove $p/q \cdot \equiv \cdot p$ and $q/p \cdot \equiv \cdot q$. But we have

$$p \cdot \equiv : (q \vee \sim q) \cdot p$$

whence replacing q by t in $(q \vee \sim q) \cdot p$ we have p; hence $p/q \cdot \equiv : p$.

This result is valid only if p and q are independent, for if p and q had constituent propositions which were truth-functionally dependent, in substituting t for q in "$(q \vee \sim q) \cdot p$" we would have to replace p by an appropriate value; whence ft would not be equivalent to p. Thus if q were the proposition $p \supset q$ (which is not independent of p) the substitution of t for this would require p to be replaced by "$(p \cdot q) \vee p$" since

$$p \supset q \cdot \equiv \cdot t \cdot \supset : p \cdot \equiv \cdot p \vee (p \cdot q)$$

In this case p/q which is $p/(p \supset q)$, would not be equivalent to p. In a similar manner it is proven that $q/p \cdot \equiv \cdot q$; consequently the proof is completed.

3.8 $p \; Ind \; q \cdot \equiv : q \; Ind \; p$

3.91 $p \; Ind \; q \cdot \supset : P(p \cdot q) = P(p) \cdot P(q)$

This is a restatement of a result obtained in section 2.

3.92 $p/q \; Irr \; q \cdot \supset : P(p \cdot q) = P(p/q) \cdot P(q)$

3.921 $q/p \; Irr \; p \cdot \supset : P(p \cdot q) = P(q/p) \cdot P(p)$

Proof of 3.92: by 3.5

$$P(p \cdot q) = P\{(p/q) \cdot q\} \qquad \ldots(1)$$

but by 3.72 q is irrelevant to p/q and by hypothesis p/q is irrelevant to q, hence p/q and q are independent and from (1) and 3.92 we have 3.92. The proposition 3.921 is obtained by interchanging p and q in 3.92.

The *conclusions* of these two propositions constitute the axiom *X*, p. 135 in Mr. Keynes' *Treatise*. This is apparently the only axiom of Keynes which is not valid generally.

3·93 $\quad p/q \; Irr \; q \cdot q/p \; Irr \; p \cdot \supset : P(p/q) = \dfrac{P(q/p) \cdot P(p)}{P(q)} \qquad [3.92.921]$

"This theorem yields as an immediate consequence the principle of *inverse probability*. Suppose q is a logical consequence of p so that $P(q/p) = 1$ [by 3.6] and suppose q has been verified. Then $P(p)$ is the prior probability before the verification and $P(p/q)$ the posterior probability to the verification. Then our result is that the posterior probability of p is its prior probability divided by the prior probability of q. The more remarkable the consequence [the smaller q is prior to its verification] the greater the increase produced by its verification." (Jeffreys, *Scientific Inference*, p. 17.)

This result depends only on p/q being irrelevant to q since if $P(q/p) = 1$, q/p is irrelevant to p, for by 3.71 a certain proposition is irrelevant to any proposition.

(c) *Bayes' Theorem*

From 3.92 we have[7]

$$\frac{P(p_1/q)}{P(q/p_1)P(p_1)} = \frac{P(p_2/q)}{P(q/p_2)P(p_2)} = \cdots\cdots \frac{P(p_n/q)}{P(q/p_n)P(p_n)} \qquad \ldots(A)$$

And since

$$\frac{a}{b} = \frac{c}{d} = \cdots$$

gives

$$\frac{a}{b} = \frac{a + c \cdots}{b + d \cdots}$$

it results from (A) for any p.

[7] In the rest of this section we shall not trouble to state the hypothesis of 3.92; besides, it is approximately satisfied in ordinary cases.

$$P(p_i/q) = \frac{\sum_{i=1}^{n} P(p_i/q)\ [P(q/p_i)P(p_i)]}{\sum_{i=1}^{n} P(q/p_i)P(p_i)}$$

If the p's are mutually exclusive and exhaustive

$$\sum_{i=1}^{n} P(p_i/q) = 1,$$

and (B) reduces to the well known *theorem of Bayes'*:

$$P(p_i/q) = \frac{P(q/p_i)P(p_i)}{\sum_{i=1}^{n} P(q/p_i)P(p_i)} \qquad \ldots(C)$$

"It follows at once that (p_i/q) can hardly ever be unity; for in the fraction on the right the denominator is the sum of the numerator and a number of other positive terms [for the p's are exhaustive and therefore cannot all be zero]. But if q has a small probability on all the hypotheses except one, p_1 say, and a large probability on that one . . . . the posterior probability of p_1 may approach unity. This is the type of inference known as the crucial test.

"Again, suppose that p_1 implies $\sim q$, so that [by 3.62]

$$P(q/p_1) = 0$$

and that nevertheless q is verified. Then (C) shows that

$$P(p_1/q) = 0$$

"This explains how the failure of a crucial test may reduce a previously plausible hypothesis to impossibility." (Jeffreys, op. cit., p. 19.)

In this theorem, as well as in all others concerning posterior probabilities, there are prior probabilities present; and these must be known before the posterior probabilities can be found. Various principles and rules for assigning prior probabilities have been proposed, all more or less unsatisfactory. The principal one is that alternatives are to be assigned equal probabilities if "we have no means of choosing between them" or when "our ignorance is equally distributed." This principle has led to a host of paradoxi-

cal results and, consequently, to alternative theories of the frequency sort.

Now on our theory every proposition, prior or posterior, has a definite probability so that no such principle is necessary. But the method of obtaining the probability by analyzing the proposition is not feasible and some other means must be devised. The method which we shall demonstrate bears an external relation ship to the frequency theories and is therefore best illustrated by some coin example.

Suppose we are presented with a coin and asked the probability of its coming up on a certain side. Given the data that the coin is fair, that the tosser can be trusted etc. etc., the probability will be near $\frac{1}{2}$. Given the data that the coin is biassed the probability will be some other value; and given sufficient data as to the forces acting on the coin at each instant, etc. we (or at least a "Laplacian calculator") could ascertain to a high degree of probability which side will come up. Now all of these probabilities are not those of (say) the proposition "Heads will come up" but of the proposition "Heads will come up given k," where k is a proposition describing some one of the various data given above; the latter is the posterior probability; the former (k) the prior probability which is still undetermined.

If we were, however, given the data that the coin was tossed up n times, we could then conclude that H(= Heads will turn up) and $\mathcal{T}$(= Tails will come up) have, on the data, n truth-values since H and $\mathcal{T}$ can be true or false n possible times. (More accurately, H/k and $\mathcal{T}/k$ will have n truth-values.) And again, if the coin comes up $n/2$ times on one side, say, the H, then we can conclude that on this data the probability of H is $\frac{1}{2}$. But this does not say that the prior probability of H is $\frac{1}{2}$. The obvious condition for this to be the case is that k, the data of the trials, should be irrelevant to or independent of H; for then $H/k \cdot \equiv \cdot H$ by the definition of irrelevance (and independence).

Our usual procedure is to infer that the number of times H comes up divided by the number of trials gives us the prior probability of H *if the number of trials is large* (*and the trials are fair*); and this has a logical justification.

It is not difficult to see that the smaller the number of trials the more dependent is it that a certain side will come up. For example, if $k = 1$ The coin was tossed up once, then H is clearly not independent of k; for if it were, every truth-value of H must be a possible value for values of k (see section 2). Now if k has the value $\mathcal{T}$, which means a certain side has come up, H can have but only one value $\mathcal{T}$ or F. But if H is neither a tautology nor a contradiction (which is the case since it is an empirical proposition) it will have both $\mathcal{T}$'s and F's in its set of truth-values. Whence it is not the case that all of its values are possible (given k as $\mathcal{T}$) and therefore k is not independent of H. Further, the greater the number of trials the less dependent H is on k: we would certainly not say that the fact of a coin coming "heads" is dependent on our tossing it up one hundred times. One hundred "tails" in succession is well nigh impossible. Hence we conclude that H/k approaches H if the number of trials is (relatively) large.

This discussion has been simplified quite a bit. The data of the trials contain more than the mere "The coin was tossed up such and-such a number of times." We must take into consideration the manner in which the individual tosses or trials were made, the physical conditions, etc. etc. If the trials are not fair, or if the data include "This coin is a trick penny with two tails" then k is of course not irrelevant and we would not have the prior probability.

4. INDUCTION

The instances of an induction or generalization can be expressed in the form $pk_i \supset q$ where p is some occurrence or experiment which is performed and k_i are the conditions under which p happens, occurs, or are the factors present, whichever the case may be; and these together, p and k_i, imply q. The generalization is $p \supset q$. Thus as an example let p = "There are clouds in the sky" and k_i various propositions like "The barometer is low etc." and q = "It will rain" The problem of induction is to find the probability of the proposition that it will rain whenever there are clouds in the sky on the data of n instances.

We have

$$(p \supset q)/(pk_1 \supset q) \cdot \equiv \cdot k_1 \vee (p \supset q) \qquad \cdots (1)$$

also

$$(p \supset q)/\{pk_1 \supset q \cdot pk_2 \supset q\} \cdot \equiv \cdot k_1 \cdot \vee \cdot (p \supset q)/(pk_2 \supset q) \qquad \text{[by 3.44]}$$

from (1)

$$\cdot \equiv \cdot k_1 \vee k_2 \vee (p \supset q)$$

Similarly

$$(p \supset q)/\{pk_1 \supset q \cdot pk_2 \supset q \cdots pk_n \supset q\} \cdot \equiv \cdot$$

$$k_1 \vee k_2 \vee k_3 \cdots k_n \vee (p \supset q) \qquad \cdots (D)$$

Thus the probability of a generalization on the data of n instances which happen under conditions k_1, k_2, $\cdots$ k_n is the probability of the disjunction of these together with the generalization. Since $p \supset q$ is what we wish to find and unknown we turn our attention to the k's.

If k_1 and k_2 are empirical propositions, i.e. propositions whose probability is less than one and greater than zero, then

$$P(k_1 \vee k_2) > P(k)$$

provided k_1 is not equivalent to k_2. Thus each instance of a generalization increases its probability but only if these are varied; and the greater the variation, i.e. the more factors taken into consideration, the greater the increase. Quite obviously, the conditions for the generalization to be certainty is that the k's should be exhaustive. This means that p happens under every possible circumstance.

In a good many cases, however, we feel an induction is justified after only a few instances. These cases are usually of the same sort as in coin-tossing where the conditions—like material of the coin, the tosser, orientation, temperature, etc.—have little relevance so that instances will occur under the most varied of conditions. (Other things being equal the technique of a good experiment is primarily to take into account extraneous influences, i.e. make them irrelevant.) In the case that one instance should

fail, say, the ith, then $P(pk_i \supset q) = 0$ and consequently $P(p \supset q) = 0$.[8]

A possible objection to (D) might occur to the reader. Isn't it the case that the k's are observed to be true and thus their probability is near certainty? One instance, then, would suffice to establish the generalization; for if k has a high probability, $k \vee (p \supset q)$ will have an equal or greater probability.

For the sake of concreteness let us return to the example at the beginning of this section and let "This is Feb. 3 and the barometer is low and etc." $= k$. Apparently this proposition has a high probability (supposing it were Feb. 3 etc.) but only apparently. What has a high probability is the proposition "k on the data that we have made these observations and our chance of being in error is small etc." This is of the form k/h which clearly does not describe the conditions whereas k does. Thus the objection holds no ground.

5. MANY-VALUED LOGICS

The system of probability from which the foregoing theory of probability was evolved, is based on the two fundamental truth-values or "categories" T and F. The so-called many-valued logics introduced by Lukasiewicz utilizes more than two basic categories.

Thus assuming that a proposition is capable of being assigned the value 1 or ? or 0 a logic may be constructed differing from the two-valued but bearing certain resemblances justifying the appellation of a logic. Similarly, by using n different truth-values n-valued logics can be generated.

We shall limit our illustrations to a three-valued system since higher valued logics manifest no additional material of any consequence. The following tables which define a system of three-valued logic, are taken from Professor Lewis' "Alternative Systems of Logic."[9]

[8] More accurately, . . . then $P(pk_i \supset q)$ will be near zero and consequently $P(p \supset q)$ will be near zero, for $P(p) = 0$ means that p is a contradiction.

[9] *Monist*, Vol. 42, Oct. 1932. The reader is recommended to this paper for a very capable and more detailed exposition of the subject of many-valued logics.

p	Np	p	q	pCq	poq	pAq	pEq
1	0	1	1	1	1	1	1
?	?	1	?	?	1	?	?
0	1	1	0	0	1	0	0
		?	1	1	1	?	?
		?	?	1	?	?	1
		?	0	?	?	0	?
		0	1	1	1	0	0
		0	?	1	?	0	?
		0	0	1	0	0	1

How these tables generate a logic is already familiar to the reader. Just as in two-valued logic, propositions from these logical constants N, C, etc. may be formed—the propositions which always have the value 1 are the tautologies of this system; and these can be used validly and consistently (within the system) for deduction.

Because of the obvious analogies with the same-named functions in two-valued logic Professor Lewis designates N as negation, C as implication, o as disjunction and so on. He then compares the tautologies of this system with those of two-valued logic (N with $\sim$, C with $\supset$, etc.) and finds that they do not all agree—i.e. a law like "p or not-p" is a tautology in two-valued logic but not in the three-valued (since $p \cdot o \cdot Np$ has the value ? and not 1 when p has this value). Thus we have a logic not coincident with that which we ordinarily use but which may have validification to some persons who rationalize in accordance with such a logic—at least this is the opinion of Professor Lewis who also contends:

"These systems [of logic] are alternatives in the sense that concepts and principles belonging to one cannot generally be introduced into the other—because of fundamental differences of categories" and that " . . . . there are no laws of logic which can be attributed to the universe or to human reason in traditional fashion."

The remainder of this section is devoted to a refutation of these conclusions with the demonstrations that the concepts and principles of one system can be introduced into another, that the

basic categories of the various systems can be reduced to those of two-valued logic and consequently propositions in three- (and higher) valued logic are merely propositions in two-valued logic.

Professor Lewis' error consists in a misinterpretation characteristic of him and can be best exhibited by means of an analogy with scales of numerical notation with radices other than 10.

It is common knowledge that our ordinary arabic notation is based on the number 10. Thus, for example,

$$381 = 3 \cdot 10^2 + 8 \cdot 10 + 1$$

Here 10 is the radix of the scale. But any number can be used just as well for a scale of notation; for example,

$$425 = 4 \cdot 9^2 + 2 \cdot 9 + 5$$

is a number in a scale of radix 9.[10] In this scale only the first 8 digits and 0 are required to represent any number. (The symbol 10 standing always for the radix.)

Now clearly

$$(1) \qquad 2 + 2 = 4$$

but

$$(2) \qquad 2 + 2 = 10$$

in a scale of radix 4. From this we do not say that (1) and (2) are propositions in alternative arithmetics; they are alternative symbolizations of the same arithmetical truth. If we were to consider 10 in both (1) and (2) as the same logical entity we would be in a similar situation as Professor Lewis and his alternative logics.

He interprets the symbols N, C, etc. as negation, implication, etc. and demonstrates that these are not identical with $\sim$, $\supset$, etc.—thus we have an alternative logic!

What is required is a *correct* interpretation of these logical

[10] The rules for multiplication and addition are of course modified. When computing with a scale of radix r we carry or borrow r instead of the usual 10. Thus using the radix 9

$$8 + 7 = 1 \cdot 9 + 6 = 16$$

For further details see any text such as Hawkes, *Advanced Algebra*.

constants in terms of the truth-values 1, ? and 0 just as the symbol 10 must be construed in terms of the radix of the scale as to whether it is 4 or 10 or whatever number.

With Professor Lewis let us consider "p has the value 1" as meaning "p is certain" or, as we would write it, $P(p) = 1$; and "p has the value 0" as "p is impossible" or $P(p) = 0$; and "p has the value ?" as "p is doubtful" i.e. "$P(p) > 0 \cdot P(p) < 1$." Now the function Np must have the value 0 when p has the value 1 and the value ? when p has this value and the value 1 when p is 0. This we obtain by defining

$$Np \cdot = \therefore \sim p \cdot \vee : \sim p \cdot \{P(p) > 0 \cdot P(p) < 1 : P(\sim p) > 0 \cdot P(\sim p) < 1\} \quad Df.$$

This reduces to $\sim p$ when p is not doubtful—which is the case as shown in the table at the beginning of this section; when the probability of p lies between 1 and 0, i.e. when p has the value ? then Np also has this value.

The function pCq we define

$$pCq \cdot = \cdot p \supset q \cdot \vee \cdot \{P(p) > 0 \cdot P(p) < 1 : P(q) > 0 \cdot P(q) < 1\} \qquad Df \cdot$$

We shall not trouble the reader with the verification of this—it is evident that pCq reduces to $p \supset q$ when p and q do not have the value?. The translation of the other functions is but a matter of detail.

The n basic categories of the n-valued logics are obtained by dividing the interval between 1 and 0 of the series of propositions, arranged according to magnitude of probability, into n-2 sub intervals; with the two end-points this gives us n different values.

To sum up, we have defined the basic categories of the various "logics" in terms of those of two-valued logic (and our probability notion) and have found that the tautologies of these logics express true propositions in two-valued logic. Thus there is but one set of logical truths.

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